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FRANCIS AVELING, 1875–1941

VOLUME XXXII JULY, 1941 PART 1

OBITUARY NOTICE

FRANCIS AVELING, 1875–1941

GRAVE is the loss that has just befallen psychology in the passing of the familiar figure of Francis Aveling.

His academic career was exceptionally distinguished. Born in Canada in 1875, he was educated at Bishop Ridley College; at McGill University; at Keble College, Oxford; at Canadian College, Rome; at the University of Louvain; and University College, London. He obtained the degrees of D.Sc. and D.Lit. (London), Ph.D. (Louvain), and D.D. (Rome). At Louvain he was awarded first class honours and was elected fellow. At London he gained the Carpenter Medal. He has held many important posts, including numerous examinationships, the Presidentship of the British Psychological Society and (up to the last) the Chair of Psychology at King's College, University of London. During the previous war, in which he served as Chaplain, he was mentioned in despatches, given the M.C. and made Commander of the Order of Christ. He was married in 1925 to Ethel, daughter of Mr S. G. Dancy of Steyning, Sussex.

Like so many other profound thinkers, he was originally inspired by metaphysics, and in particular by the tremendous question as to "What is the ultimate nature of Reality?" But then forthwith, like all his predecessors, he found himself up against the hardly less baffling obstacle as to how such reality admits of being *known*. Obviously, however, all knowing consists in processes of cognition. And after this fashion he necessarily moved on from the field of epistemology to that of psychology.

Now of all the psychological resources for this critical function of knowing truth, by far the most widely credited throughout history has been provided by what have been called 'universal' ideas and propositions. These, as their name implies, extend their scope throughout time, space, and even imagination; whereas the knowledge afforded by sensory percepts is rigidly confined to here and now. What is still more important, the ideas have been credited with the cardinal virtue of being unchangeable; whereas the percepts are notoriously shifting from moment to moment.

Accordingly, it was these alone trustworthy ideas that supplied Aveling with his first main object of investigation. And he led off by examining their connexion with those more or less faint and fragmentary perceptual reproducts that are commonly called 'images'. This connexion proved to be unexpectedly loose and wayward. Far from constituting the essence of the universal ideas, the images were in most cases absent. Indeed, they turned out eventually to be not indispensable even for thoughts of particular objects. Otherwise expressed, any kind of thought could be 'imageless'.

Even more vital, however, than this problem as to how these ideas are constituted, is that as to how they are created. And on this matter Aveling went ahead of all his predecessors. For these had supposed that such ideas could only arise by way of 'abstraction', at most 'combination', from antecedent experience, and therefore could never exceed the confines of this. For example, the universal idea of man was taken to be nothing more than an abstraction from more or less numerous percepts of individual men.

Aveling, on the contrary, found that the ideas were frequently of such a nature as neither had been, nor possibly could be, given in previous experience. In such cases they derive, not from any abstraction, but from the totally different process of 'educing correlates'. He writes:

> The presenting of any character together with any relation tends to evoke immediately a knowing of the correlative character. Thus one tends to think or imagine, say, a line half the length of a given line if the relation 'half' be in the mind.

To such eductive processes he attributes, for instance, the construction of the ideas of 'immortality' and of 'God'. He has been careful, however, to add some qualifications. Although such correlates do not necessarily *consist of* any parts or parcels of previous experience, it must needs at least be *generated by* these. Moreover, he has always maintained that the power of thinking such correlates neither proves nor disproves the existence of any corresponding reality.

Naturally enough, however, Aveling's pursuit of truth was not restricted to the aforesaid analysis of thought. It covered also the other great cognitive faculty, which is sensory perception, and which in point of fact is much the more complex of the two. To its manifold constituents and phases he and his numerous pupils devoted a long series of brilliant experimental researches. One notable fruit was the observation which he and R. Cattell made of the transition from the original merely 'felt' sensory experience to the knowledge of its nature. Another outstanding discovery in sensory perception was the previously mentioned process of educing correlates. By way of this process he was able to explain such fundamental events as the appearance of 'substance' and of 'force'.

Here we are led over to a new sphere of Aveling's main activity. It is one which, although still replete with philosophical interest, is no longer cognitive, but instead deals with the human Will. And at this point his achievements may be said to reach their climax.

He appears to have set out from the awesome problem as to the nature and origin of the idea of 'cause'. First of all, he confirmed the discovery by Malebranche (and afterwards by Hume) that nothing of this sort is disclosed in bare 'felt' sensory experience. As an illustration, he takes the case where a person seemingly perceives that 'water rusts iron'. Here, the person does not actually see any causation, but solely certain sequences of shape and colour. Causation can indeed be actually felt, he finds, but only in one privileged case. To understand this, he says, it is needful to distinguish two fundamentally different mental operations. One of these is properly entitled that of 'willing' (it includes resolving, deciding, determining and the like). The other operation he calls 'conation' (it takes in such things as trying and striving). He contrasts the two operations by noting the fact that "a decision made in a moment may have as its consequence a series of conations which last a lifetime". Now, it is between these two that causation (properly so called) was found by Aveling and his pupils to be actually felt and known to be so; on the direct evidence of consciousness, a person tries *because* he wills to do so.

From this primordial awareness of oneself as causing, and as simultaneously being caused, Aveling was able to proceed to other profound problems. For instance, he was able to show how the mind manages to escape from the toils of solipsism (see above). For when once any person knows himself to be causative, it is an easy step for him to regard other objects as being similarly so. "This process", he argues, "is one of applying one relation by analogy to another." It is, in fact, once more a case of educing a correlate.

So far our summary exposition of the great work of Aveling may to some readers seem to have been more philosophical than psychological. But this would be a misapprehension. In point of fact, he has been rather chary of discussing philosophy. He only claims to supply an extremely potent *approach* to this. Whether or not he has succeeded in substantiating this claim, his facts in themselves remain strictly psychological, as do his methods of obtaining them. Far from suffering from any philosophical or other narrowing bias, his work is singularly comprehensive and informative. From his multitudinous but scattered publications there could be constructed a General Psychology unsurpassed. Pending such a desirable consummation, we must content ourselves with citing his publications in their present forms.

Among his books may specially be mentioned the following: *On the Consciousness of the Universal and the Individual; Confirmations expérimentales d'une théorie du processus cognitif; Arnoul the Englishman* (novel)*; The Immortality of the Soul; Science and Faith; The God of Philosophy; Modernism; The Philosophers of the Smoking-Room; The Spectrum of Truth* (with Rev. A. B. Sharpe)*; Directing Mental Energy; The Psychological Approach to Reality.*

To the preceding volumes must be added his *Expository Essays in Christian Philosophy,* besides numerous contributions to periodical magazines.

Still more valuable, if in general less accessible, have been his lectures and the manifold researches made under his inspiration.

Even less available to the general public, of course, but in itself most precious of all, has been the conversation so generously bestowed upon his admiring and affectionate friends.

C. SPEARMAN.

OBITUARY NOTICE

SIR HENRY HEAD, 1861–1940

Henry Head died in his eightieth year on the eighth of last October. As physiologist, neurologist, psychologist and physician, publishing brilliantly original research over an uninterrupted period of some forty-five years, he was one of the most distinguished workers in these combined fields whom Great Britain has ever produced. During the last Great War, he gave up his consulting practice and went to live in hospital, devoting all his time to research on, and to the treatment of, his soldier patients, and to various medico-psychological matters affecting the Fighting Services. But only a few years after the conclusion of the peace he fell a tragic victim to that incurably fatal disease of the central nervous system, paralysis agitans. He had already delivered in 1920 the Hughlings Jackson lecture on his studies of cerebral injuries in war-time; and he was just able in 1926 to publish his two monumental volumes on *Aphasia and Kindred Disorders of Speech.* Thereafter he lived a carefully planned life in retirement near Reading, devotedly nursed by his highly cultured and vivacious wife (née Ruth Mayhew) whom he had married in 1904 and who, to his irreparable sorrow, predeceased him in 1939. Both before and after their marriage, she had been an able literary critic of his scientific publications, to whom he always submitted them. To her, "without whose touch these strings would have been mute", he dedicated his volume of poems, *Destroyers and Other Verses*, published in 1919—a collection of original verses and of translations from German poems. His home at Hartley Court became a place of pilgrimage for many of his former colleagues resident both in this country and abroad. To the end he retained his old mental vigour and exceptionally wide interests, despite his slow but progressive loss of voluntary muscular power. He received the honour of knighthood in 1927.

Head came of Quaker stock. He was born in 1861 at Stamford Hill; his father, also a Henry Head, was a member of Lloyd's. From Charterhouse in 1880, where he obtained a scholarship, he entered Trinity College, Cambridge, where he also won a scholarship and, towards the close of his career, was elected to an honorary fellowship. After obtaining his B.A. degree with first class honours in Physiology in Part II of the Natural Sciences Tripos, he studied for two years in the German University of Prague. At Cambridge he had been greatly impressed by the views and teaching of Walter Gaskell; to Prague he went in order to work under Ewald Hering; later, at the London Hospital, he came under the lasting influence of Hughlings Jackson. These remained to the end his three greatest heroes in the fields of contemporary physiology and neurology, "who", as he said, "have most influenced my methods of work and thought": to their memory he dedicated his last publication—his two volumes on aphasia.

When only twenty-three years old, he carried out in Hering's laboratory physiological experiments upon the action of the vagus nerve in respiratory movements, recording the latter by the use of a strip of the diaphragm (known henceforth as 'Head's strip'), which became a standard method of investigation. These experiments were published in *The Journal of Physiology*, 1889. In the autumn of 1886 he was back at Cambridge, investigating with characteristic thoroughness his first case of aphasia at Addenbrooke's Hospital and completing his anatomical and physiological studies for a medical degree in the University laboratories. He then entered University College Hospital for his clinical work, and took the Conjoint Diplomas of the Royal Colleges of Physicians and Surgeons and the M.B. degree of Cambridge in 1890. He became M.R.C.P. in 1894 and F.R.C.P. in 1900.

For the M.D. degree at Cambridge, which he took in 1892, Head submitted one of the most important theses which can ever have been written for the purpose: "On disturbances of sensation, with especial reference to the pain of visceral disease." This was published in an elaborated form in the pages of *Brain* for 1893, 1894 and 1896. The investigation began with a study of 'referred' cutaneous pain or tenderness associated with disease of the various viscera, the irritation due to which, Head supposed, opened up a nervous path normally inhibited by the action of cutaneous sensibility. He was led on to study the distribution of herpes zoster, an eruption of the skin associated with disease of the posterior root ganglia of the spinal cord. This distribution, the neurological implications of which had been simultaneously realized by James Mackenzie, was found by Head to coincide closely with the tender cutaneous areas which he had already traced in visceral disease. The areas affected became henceforth known as 'Head's areas'; their nerve supply was traced to the various segments of the spinal cord. These discoveries spurred him to investigate the nerve supply of the viscera, the cutaneous distribution of the posterior spinal roots, and, with Campbell, the primary and secondary, central and peripheral, nervous lesions occurring in cases of herpes zoster. He subsequently investigated the hallucinations, etc., occurring in visceral disease, the results of which he gave in his Goulstonian lecture of 1901 to the Royal College of Physicians, London, entitled "Some mental symptoms associated with visceral disease in the sane", and published in *Brain* of that year.

A characteristic of Head's genius was the magnitude of the problems which he chose for attack. His other three *magna opera* consisted in an investigation of the disturbances and return of cutaneous sensibility after section of the external cutaneous and radial nerves, and excision of a short length of those nerves before suture, in his own left arm; in tracing, by means of clinical examination, the paths of sensory impulses throughout the cord and brain-stem to the optic thalamus and the cerebral sensory cortex, and in elucidating the functions of the thalamus and sensory cortex; and in a profound study of the nature and varieties of aphasia.

Another of his characteristics was his wise choice of able collaborators and advisers—W. H. R. Rivers and James Sherren in the first of these three investigations, Gordon Holmes and Theodore Thompson in the second, Elliot Smith and F. C. Bartlett in the third, and George Riddoch in a war study of the striking phenomena of the 'mass-reflex' occurring below the level of a lesion of the spinal cord. Most of the results of this series of truly epoch-making results were published in *Brain*, between 1908 and 1918. Of this journal Head was editor from 1910 until 1925. In it in 1915 he published Hughlings Jackson's far-sighted but long neglected and scattered papers on aphasia, dating from 1864 to 1893. Later, in his own great work on the subject, Head applied some of Hughlings Jackson's broad views (as the latter had not) to actual cases of aphasia. In 1919 many of Head's neurological papers were collected and published, in conjunction with his collaborators, in two volumes entitled *Neurological Studies*. His work with Gordon Holmes on "sensory changes from lesions to the sensory cortex and other cerebral lesions" formed the subject of the four Croonian lectures which he delivered before the Royal College of Physicians in 1911.

Yet another of Head's characteristics was the thorough and systematic manner in which he first prepared himself, and later reserved time, for his research work. After receiving his first medical qualifications, he filled house appointments at University College Hospital and at the Victoria Park Hospital for Diseases of the Chest. In 1894 he served as clinical assistant to the Lancashire County Asylum at Rainhill. Then he was appointed Medical Registrar to the London Hospital, becoming Assistant Physician there in 1898, later full Physician and finally

Consultant Physician. In the methods of experimental psychology he received his training at the hands of W. H. R. Rivers, while working with him in testing the recovery of sensibility of the skin after the section of two nerves in his own arm. Between 15 April 1903 and 13 December 1907, he had 167 days' sittings with Rivers in the rooms of the latter at St John's College, Cambridge, standard and improved tests of cutaneous sensibility being applied by Rivers to the affected regions of his skin. Every Friday or Saturday during this period, he used to travel from London to Cambridge, where rooms in college were allotted to him for the week-end. In later years he always set aside one day in each week for his research work: neither private practice nor any other engagements were ever allowed to desecrate this day.

His medical colleagues were not slow in recognizing the high standard of his research. From the Royal College of Physicians in 1897 he received the Moxon Medal, and from the Royal Medical and Chirurgical Society in 1903 he received the Marshall Hall Prize, conferred respectively every three and five years for the most distinguished and original work in clinical medicine carried out during those periods. He received the LL.D. degree at Edinburgh and the M.D. degree of Strasbourg. He was elected a Fellow of the Royal Society in 1899, served on its Council from 1915 to 1917 and as a Vice-President from 1916 to 1917, was awarded a Royal Medal of the Society in 1908, and delivered its Croonian lecture in 1921.

Head was a thick-set man of good stature, and of florid appearance which, with his short pointed beard and gold-rimmed spectacles, gave him somewhat the appearance of a foreigner. Indeed, he used to say that in his younger days he was frequently mistaken for a German by Englishmen when he worked in Prague. He spoke German remarkably well, and French also; it was characteristic of him that on a holiday he used to learn the most irregular of French verbs while dressing. He spoke, often excitedly, in a high-pitched voice. His thoughts were apt to vault, as it were, over obstacles which were non-existent only for him, so that it was not always easy for those of less intellectual brilliancy to follow the swift train of his expressed ideas. Few men have surpassed him in exuberance of imagination and in untiring energy: the irrepressibility of his genius was at times almost overwhelming. In him was to be found a rare admixture of intellect and emotionality, a combination of the characteristics of the scientist and of the artist. Yet he never allowed the brilliance of his imagination and the lightning speed of his conclusions to escape the later control of his scientific acumen. To the end he was intensely interested in drama, music, painting and literature, especially in poetry which he loved discussing with philosophically inclined poets. He was himself endowed with great dramatic talent, especially evident when he was portraying the voice, facial expression, gait and other movements of a sufferer from some nervous malady. Indeed, he would throw himself so wholly into the 'part' which he was assuming that one who did not know him well might suppose that he was suddenly stricken by the disease that he was imitating. He hated dishonesty and self-seeking. If he—rightly or even wrongly—suspected them in another, the latter at once forfeited his inestimable friendship. He was ever ready to encourage his abler pupils and to help even strangers in their personal difficulties. Always attracted to the wider problems of human affairs, he gave unstinted support to projects for the improvement of medical organization, its status and education. Thus he took an active part in the amalgamation of various independent medical societies to form the present Royal Society of Medicine, and he gave the University of Cambridge every encouragement to establish a Diploma in Psychological Medicine which, however, was allowed to lapse, for reasons of economy, after the end of the last Great War.

So wide was the extent of his scientific investigations that only a few of their most outstanding results can be mentioned here. Head's heroic researches on cutaneous sensibility resulted in a clear separation between 'deep' sensibility (to heavy tactual and painful pressure) and the two varieties of superficial sensibility which he came to distinguish as 'protopathic' and 'epicritic'. 'Deep' sensibility, he found, remains after the section of the cutaneous nerves: the afferent nerves subserving it run mostly with the efferent (motor) nerves. The features of protopathic sensibility were its 'spotted' distribution, its radiating, vivid, 'all-or-none' character, vague localization, loss of tactual discrimination and loss of adaptation. Touch was lost, save by mechanical stimulation of the hairs of the skin. Sensibility to hot and cold stimuli was retained, but warm and cool stimuli evoked no sensations of temperature. Protopathic sensibility was the first to return, followed slowly by epicritic sensibility with a gradual diminution of the characteristics of the protopathic form. In a small area of Head's arm epicritic sensibility was found at one stage, unaccompanied by the fundamental features of protopathic sensibility. With the reappearance of epicritic sensibility cool and warm sensations could be evoked, light touch began to evoke sensation, and graded sensibility, accurate localization, tactual discrimination, and adaptation returned.

These astonishing results provoked others to repeat Head's experiment, but usually in a more or less ambiguous and non-comparable form, and so producing variously different results. On the whole, his conclusions are accepted to-day by his fellow-neurologists, although doubts are entertained as to the sharp line which he drew between the protopathic and epicritic systems. More doubtful, however, is the evolutionary interpretation which he gave to the relation between these two systems. Here, doubtless, he was influenced by his collaborator, Rivers. Just as the latter, in considering the relation between instinct and intelligence, had supposed that in the evolution of the animal kingdom instinct arose first and intelligence was later grafted on it, so Head believed that the protopathic was phylogenetically the sole earlier form of cutaneous sensibility, to which subsequently the epicritic became added. Little evidence in support of this view can be adduced. It is probably as incorrect as is Head's view that the widespread, temporary, 'mass reflex' observed by him and Riddoch at a certain stage after spinal lesions represents a once primitive phase of the normal, more restricted, spinal reflexes.

Coming next to his study of the grouping of afferent impulses from the skin, subcutaneous tissues, muscles, tendons and joints, as the nerves enter and ascend the spinal cord, we meet with equally striking discoveries and acquire a clearer and surer knowledge of what was previously obscure or uncertain. Within the cord, the temperature impulses arising from his protopathic and epicritic systems immediately combine, the heat (protopathic) and warm (epicritic) together and the cold and cool together, so that injury to one spinal (or higher) area will result in the loss of one, whereas injury to another will result in the loss of the other, of these two pairs of sensations. They cross at once to the opposite side of the cord. A similar combination and crossing of tactual impulses—including localization, but excluding spatial discrimination (compass points) and the tactual appreciation of form and size—also, but far less immediately, occur after entry into the spinal cord. With intermediate speed, after entering the cord, the painful impulses of the peripheral systems of sensibility similarly combine and cross. These three separate tracts (for temperature, touch and pain) thus sooner or later cross to the opposite side in their ascent of the cord; whereas the tracts subserving (*a*) recognition of passive movement and posture from the deep system, and (*b*) spatial discrimination and appreciation of form and size from the epicritic, cross in the bulb just above the cord, and become grouped afresh, the sensory fibres of both groups travelling to the optic thalamus and thence to the cerebral cortex. The impulses concerned with the recognition of movement and posture part company in the bulb with those concerned with spatial discrimination and appreciation of size, so that injuries above this level cause loss of one of these pairs without loss of the other. Many of the non-sensory fibres of the former group do not cross in the bulb but proceed by the direct cerebellar tract to the cerebellum.

With Holmes Head carried out most important work on the recognition of impulses in, and on the functions of, the optic thalamus. He showed how the functions of its essential organ are controlled by inhibitory impulses passing from the cerebral cortex to the lateral nucleus of the thalamus and how, when these impulses are removed by injury, affective over-responses result. When such injury is unilateral, sensory stimuli capable of arousing pleasure or displeasure may now evoke the most extreme delight or the most intolerable discomfort when applied to the opposite side of the body. But we are not justified in supposing that the essential organ of the thalamus is, as Head believed, "the centre of consciousness for certain elements of sensation". Under the above-mentioned conditions, the thalamus still preserves connexions with the cerebral cortex, and we have good reasons for thinking that without the activity of wide regions of that cortex consciousness in any form is impossible. No "centres of consciousness" are conceivable.

Head came to realize that "sensation, the final end" of the process of integration of sensory impulses, implying a change "from a complex to a simpler more specific grouping...assumes forms simpler than any sensory impulses". He concluded, from his study of lesions of the sensory cortex, that such lesions produce usually no true rise in threshold (especially the tactual and spatial threshold), but an irregularity and uncertainty of response (quite independent of the strength of the stimulus), making determination of the threshold often impossible. He regarded the sensory cortex as "an organ of local attention" and as being concerned especially in relating the new to the old—in discrimination, balance and clearness. He came also to the conclusion that it "registers and retains the physiological dispositions produced by past events", forming unconscious "organized models of ourselves" to which he gave the somewhat inappropriate name of 'schemata', by which sensations come to be charged "with a relation to something that has happened before". By means of such ever plastic models or 'schemata', nonconscious standards are constructed, only in relation to which "every change in posture, movement or localization can enter consciousness", and on and by which it must be first recorded. These models or 'schemata' are variously destroyed in lesions of different parts of the sensory cortex.

In formulating this important conception of unconscious 'schemata' Head had reached the equally important conclusion that conscious 'images' of past movements can no longer be regarded as the basis of our recognition of bodily posture and movement. Such recognition depends both on the integration of afferent impulses with the unconscious 'schemata' and on the activity of those 'schemata'. Nevertheless, despite his recognition of the continual growth in elaboration and complexity of these 'schemata', Head allowed himself to speak of them as the 'storehouse' of past impressions: thus he was not yet wholly emancipated from the standpoint and terminology of an older psychology, which regarded images as being 'stored up' in the brain.

Head reached the peak of his psychological maturity in his last and perhaps most striking research—into aphasia. He began it when neurology was dominated by the 'diagram makers' who were intent on explaining aphasia and kindred disorders in terms of injury to various nerve 'centres' in the brain—visual and auditory word centres, glosso- and cheiro-kinaesthetic centres, etc., and to commisural nerve fibres connecting with and 'associating' such centres. They were able to uphold these preconceptions and prejudices by neglecting to make, or by failing to take into account, various clinical phenomena which contradicted them, or by their imperfect conduct of *post mortem* examinations. Like his master Hughlings Jackson, Head insisted on the difference between localization of the cerebral damage that destroys speech and localization of speech itself; the conception of localized 'centres' for normal mental functions rightly seemed to him an anachronism in modern cerebral physiology. Like Jackson, too, he realized that speech is a function of mental activity and that it presents therefore a problem primarily of a psychological nature; and that the loss of speech is fundamentally the loss of ability to formulate a proposition—which is more than a mere sequence of words. As Jackson had pointed out, words used non-propositionally or emotionally, e.g. "God bless me!" "Oh dear!" may still be uttered by the aphasic; he may also still be able to *receive* propositions. Speechlessness does not imply wordlessness, nor are words essential for all thinking.

Head excelled in bringing the precise, systematic methods of experimental psychology into clinical use. In the study of his cases of aphasia he stressed the importance of keeping the order of presentation of his test material constant, of proceeding from the easier to the more difficult tests, of avoiding fatigue, boredom or loss of temper during the examination of his patients, and of repeating the tests on different days and at different stages of recovery owing to the variable results thus obtained. He realized the importance of the relations subsisting between physician and patient, one physician—like an interviewer confronting a candidate—obtaining different results from another. He was not content merely with recording the patient's responses, but also observed and interrogated him in regard to the facility of those responses and to the manner in which he carried them out. He found it impossible to classify his cases in any tabular form.

When he entered on the study of aphasia, Head quickly recognized that the disorders of speech are not due to the loss of images; these might be retained, although no longer available for voluntary use as part of "the symbolic mechanism of language". He also recognized that the 'pure' psychological tests, devised to investigate the analysed elements of sensation, perception, movement, etc., which he had employed in his previous researches, were now quite unsuited for his purpose. Like other workers in 'applied' psychology, e.g. in occupational selection, he employed 'sample', in place of 'analytic' tests—more 'complex' tests which imitated the experience and conduct of everyday life. Head realized that the sum of the separate parts was very different from the whole in any presented test situation. As he wrote in 1924 in one of the two papers which he published in this *Journal*, one or more functions associated with consciousness, when lost, "cannot be said to be the 'elements' out of which general consciousness is 'composed': they form rather forces in a psychical interplay" (p. 182). So, too, he insisted that the defects of aphasia cannot indicate the elements composing the use of language. They are to be described rather to a failure of the functions of "symbolic thinking and expression". He was careful to describe his tests in such detail that they could be reapplied by others, and to base his conclusions not on preconceived notions but on the recorded results of his observations. He came to distinguish four types of aphasia: (i) 'verbal', where the meaning and value of words are retained, but their formation (in external rather than internal speech) is affected; (ii) 'syntactical', where the syntax suffers through disorder of balance, time-order and rhythm, of articulation; (iii) 'nominal', where words (and other symbols) have lost their immediate meaning and cannot be fitted to objects; (iv) 'semantic', where the loss of meaning is far more profound, general ideas of what the patient has been ordered to do, orally or in writing, or of what he has read, being no longer possible, and the ability to combine details into a consistent whole being lost. Although he admitted that broadly different cerebral lesions were associated with these four types of disturbances of speech, Head insisted that the use of language in speaking, reading and writing cannot be ascribed to the function of separate 'centres'. As he expressed himself in 1921 in the other article which he contributed to this *Journal*, "there is no reason to suppose that any groups of cerebral functions correspond to...'reading' or 'writing' or even to 'speech' itself. Any lesion which gravely disturbs 'speech' affects activities which are not usually classed with the use of language..." (p. 181).

Throughout his various researches Head had observed how closely neural and mental functions are dependent on the bodily and mental health of the individual. For example, headache, fatigue, or worry will cause the abnormal sensibility (e.g. numbness or loss of sensation) of the hand after cerebral injury to be temporarily replaced by the cessation of its very existence, owing, as he supposed, to a temporarily dormant condition of the cortical 'schemata'. Cold weather, during the recovery of the sensibility of his own arm after neural section, would throw its condition back from the epicritic to the earlier protopathic stage. Especially through toxic or slight narcotic influences, through so-called 'shock' immediately after the lesion, the reflexes observed in the spinal or decerebrate animal, or in a human patient suffering from a severed cord, will differ profoundly from their usual or later character. During the illness of a child previously liable to bed-wetting, this cured weakness may recur; owing to a cold or an internal 'upset', an expert skilled in an acquired act will lose his skill. All such lapses were summarized by Head as due to the loss of what he termed physiological 'vigilance'—a somnolence of the central nervous system, involving, apparently, both a loss of local alertness and a loss of control of the unconscious higher reflexes over the lower reflexes, and of conscious processes over the higher reflexes. However obscure and perhaps too comprehensive this conception of 'vigilance' may be, Head performed a useful service in bringing these various phenomena together and thus in calling attention to them. In this, as in so much of his other, research he performed pioneer work which it must be left to future generations, always admiring and mourning his loss, to refine and to complete. *His* highest-level 'vigilance' can seldom have been surpassed!

C. S. MYERS.

TOPOLOGICAL PSYCHOLOGY AND NEUROGRAPHICS

BY MAXWELL GARNETT

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I. INTRODUCTION

WHEN William McDougall, in one of the last of his writings (1), reviewed the 'Dynamic Principles of Gestalt Psychology' he 'wholly approved' of many of its leading features. At the same time he found much to criticize in the work of Wolfgang Köhler (2) and Kurt Koffka (3), but his criticisms were mild in comparison with his objections to certain views of Kurt Lewin (4). McDougall had not then seen Lewin's book on *Principles of Topological Psychology* (5). From this volume Lewin has omitted the so-called 'vector psychology' against which McDougall protested most strongly.

What remains is an attractive essay, clearly and forcibly written in simple language. It is admirably translated. And it provides a welcome introduction to the spatial representation of psychological phenomena. Lewin tells us, in his preface dated May 1936, that topological psychology began, more than ten years earlier, when it occurred to him "that the figures on the blackboard, which were to illustrate some problems for a group in psychology, might after all be not merely illustrations but representations of real concepts". Similar diagrammatic representations of real concepts had, however, already been derived from McDougall's 'hormic' psychology and published in England in 1921 (6). While the 'real concepts' represented in Lewin's diagrams are "quasi-physical, quasi-social and quasi-conceptual" ((5), p. 24), the real concepts represented in the earlier diagrams were simply physical: in fact, neurological configurations or neurograms. Yet, in view of the criticisms levelled by McDougall against Lewin's earlier volume, it is surprising to find how many and how close are the resemblances between Lewin's 'principles of topological psychology' and the 'neurographics' derived from McDougall's own work.

In this paper I use the word 'neurographics' to describe the methods of 'hormic' or neurological psychology which I employed in *Education and World Citizenship* (6) and in a recent shorter version (7) of that work. Neurographic psychology seeks to describe the working of the mind in terms of brain processes. We may, perhaps, know more about the working of the mind than of the brain. But, as Sir Henry Head showed in his work on *Aphasia* ((8), vol. I, pp. 541 and 549), the states of mind revealed by introspection are 'categorical abstractions'. Although sensations, images and the like appear when the mind examines its own activities "they are not portions of an integrative reaction". The final thought follows from the brain processes but not necessarily from the intermediate thoughts. "The field open...to introspection is limited to the phenomena of consciousness." But many psychological processes are not included in these phenomena. Head's researches into the effects of injuries to the brain "reveal a multitude of processes which participate in the response", or influence the end of a train of thought, "although they do not form part of the continuum of consciousness and are not normally open to introspection". The same is true of processes described in Economo's account of *Encephalitis Lethargica* and its effects, both physiological and psychological (9).

Moreover, as T. H. Huxley pointed out, "there can be little doubt that the further science advances, the more extensively and consistently will all the phenomena of Nature be represented by materialistic formulae and symbols". By using them for the purpose of our reasoning we lessen the likelihood of mistaking the meaning of a proposition between the moment when we meet it for the first time as the conclusion of one syllogism, and the moment when we come back to it as the premise of another syllogism. Our reasoning is more likely to be accurate on this account: our conclusions will follow more certainly from our premises, and a comparison of these conclusions with the observed facts will show us more plainly if our premises or hypotheses are wrong. And if our conclusions accord with all the observed facts, we shall feel the more sure of the truth of our premises and of any other conclusions we may draw from them.

For these reasons, neurographics employs 'materialistic formulae and symbols'. Like topology, it is concerned with the spatial representation of psychological phenomena. Both neurographic and topological psychology employ 'real concepts' for the purpose of this representation[1] and both make use of diagrams to represent[2] their 'real concepts'. The real concepts of neurographics (neurograms and their excitement, synapses and their thresholds) have more in common than have those of topology (life space and its regions, points and paths, boundaries and dynamic walls) with the psychological phenomena which they directly represent. But, in the second stage of representation, the advantage is with topology: its diagrams and its 'real concepts' are more alike than are those of neurographics.

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[1] "The system of concepts capable of bringing together the different fields of psychology in an empirical manner would have to be...suitable for the representation of general laws and of the characteristics of the individual case" (Lewin(5), p. 5). And again, "a set of concepts by means of which one can represent psychological reality" ((5), p. 6). Such a concept is "the psychological life space" ((5), p. 12).

[2] "What is meant by psychological life space and what must one take into consideration in order to represent it" ((5), p. 18).

Again, both topology and neurographics consistently maintain unequivocal relations to the observable psychological data. Both represent the person and the environment—or so much of it as affects the person at the present moment—in common terms as part of one situation. Both represent causal interrelationships having compulsory consequences. Both representations are susceptible of mathematical treatment.

Many of the psychological situations and events represented topologically in Lewin's book have also been represented neurographically. Some examples are given below. In every case but one[1] the two representations are mutually consistent and have the same psychological consequences. Lewin's book also contains topological representations of other psychological situations which have not been, but might easily be, represented neurographically.

In topological psychology, or at least in Lewin's book with which alone we are here concerned, the representation of causal interrelationships is confined to psychostatics.[2] In neurographics, it extends to psychodynamics: the study of *change* in psychological situations or of the *flow* of consciousness. In this latter field topological psychology is content to be merely descriptive. It does not attempt to foretell events. Although Lewin refers ((5), p. 82) to 'dynamic laws', he gives no examples of what they are or how they operate. But neurographics has formulated five laws, three of them concerned with psychodynamics outside the realm of will. These three laws are cited below. Many examples of their working in everyday life are given in (7), pp. 90–93, 260–266.

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[1] In this one case Lewin, on p. 181, represents the person as having the same motor as perceptual relationship to his environment. The person's links with his environment whether efferent or afferent are confined by Lewin's diagram to one and the same 'motor-perceptual region'. Lewin's representation would imply that when some incoming sense impression influences the 'outer regions' of the person and they in turn affect the 'inner regions', these latter cannot react directly upon the environment but the chain of cause and effect must reverse its path and go out by the way it came in. Neurographics could not allow such a breach of the law of forward conduction. See McDougall ((10), p. 132).

[2] Lewin does not use this word; but he makes many references to (psycho-)dynamics. The words psychostatics and psychodynamics occur in the first account of neurographics. See (6), p. 65.

the representation of the psychological life space only to the extent and in the manner in which they affect the individual in his momentary state. We express this by calling them quasi-physical...quasi-social...quasi-conceptual facts ((5), pp. 24–27) = Among the active neurograms and their connexions which, apart from the person's 'will', determine his behaviour are his neurograms of physical things, of other persons, and of abstract conceptions. They are all actual, not 'quasi', facts. But they correspond to his knowledge of things, and (in so far as that knowledge is incomplete or inaccurate) not exactly to the things themselves.

A region in the life space ((5), p. 93) = A neurogram or a system of connected neurograms ((7), p. 57).

Each region which has the character of a thing ((5), p. 107) = The neurogram of a thing ((7), p. 61).

5. [*If*] *the field is unstructured with reference to cognition,* [*the result is*] *one's ignorance of the situation* ((5), p. 133) = "the more perfectly a man's neurography is organized [or 'structured'] to represent the objects in the world as well as the relations between those objects, the more extensive and the more accurate will his knowledge be" ((7), p. 84). For instance, to take one of Lewin's examples, I may want to go from the railway station in a strange foreign city to a certain house without having a map and without knowing where to ask for information. My ignorance of the situation means that, although both the station where I am and the house where I would be are represented among my neurograms, the route from the one to the other is not so represented. Therefore the excitement of the neurogram of my starting point, as I think of my predicament at the station, does not find its way to my neurogram of the house by any neural path of low resistance which corresponds, element by element, to any actual way from the station to the house. Thus I cannot think out any real way from my starting point to my goal; and, since I cannot find my way in thought, I cannot do so in fact. Reasoning solves the problem, and neurographics describes in detail how the process works. It is illustrated by the second diagram on p. 28 below. In brief, I concentrate upon a particular partial aspect of the problem before me. What should be my first move? Among the thoughts which then present themselves is that of walking out of the station and straight on until I meet someone who will understand and direct me. Again I concentrate on this idea to the exclusion of all alternatives. Action follows thought; and, sure enough, I find the help I need. The information thus obtained makes a neurographic link, representing the true facts, between my neurograms of the station and of the house.

Since I can now find my way in thought from the one to the other I can also move my body across the corresponding gap and reach my destination.

6. *Overlapping Regions.... The cases in which two regions overlap... play an important role in psychology* ((5), p. 137) = "The same neurones and systems of neurones may form part of several different neurograms, just as the same movement may take place in several different reflex or habitual acts" ((7), p. 54). For example, a zoologist's neurograms for a dog and for a cat overlap in his neurogram for a carnivore. The important part played in psychology by overlapping facts, and by the neurograms that represent them, has been examined at some length in connexion with the organization of thought ((7), pp. 195–214).

7. *The breaking up of regions into subregions...can easily be treated mathematically* ((5), p. 155) = For a mathematical treatment of systems of neurograms and the elements into which they can be broken up, see (6), pp. 206–12.

8. *It is sometimes doubtful whether we should represent a psychological fact as a point or as a more-dimensional region* ((5), p. 110).... *In psychology the point is equivalent to a region which is not structured into parts* ((5), p. 112) = "An ESSENCE is such a particular partial aspect of a fact as can only be thought of as a whole or not at all: an all-or-none fact of the world of experience" ((7), p. 198). To such essences, always used in reasoning, correspond 'elements' of neurograms ((7), pp. 195, 227, 252).

9. *Individual Differences* ((5), p. 182). *Even when from the standpoint of the physicist the environment is identical or nearly identical for a child and for an adult, the psychological situation can be fundamentally different* ((5), p. 24) = "If I know nothing, or not enough, of" a fact in my environment, it "is represented in my neurography by a neurogram from which elements corresponding to certain essences are missing. The fact is short-circuited" ((7), p. 227). To borrow an illustration from William James. "Suppose I say when offered a piece of cloth 'I won't buy that; it looks as if it would fade', meaning merely that something about it suggests the idea of fading to my mind—my judgement, though possibly correct, is not reasoned, but purely empirical." My neurogram of the cloth is 'short-circuited' because it lacks elements corresponding to the chemical constitution of the dyestuff, so that I cannot give the reasoned judgement that the cloth will fade because I know the dyestuff to be chemically unstable.

"Disputes between two or more people are often due to ignorance: to

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the imperfect correspondence of their neurographies with the realm of facts and therefore with one another. Indeed, if we take account of affective as well as of cognitive neurograms, all disputes may be attributed to neurographic differences between the disputants" ((7), p. 228).

10. *The person is a differentiated region in the life space...a highly differentiated object* ((5), p. 166) ≐ "The central elements of the" neurography that is "best for the individual and for the community" to which he belongs "include those corresponding to the self and the self-regarding sentiment" ((7), p. 221). "The dominating position is in most cases occupied by, and the controlling function exercised by, the highly complex sentiment peculiar to man, the sentiment of self-regard" (quoted from McDougall in (7), p. 77). "Since some idea of my self enters into all my purposes the neurogram of my 'self-regarding sentiment'...forms part of my future interest-system" ((7), p. 157), i.e. the system of neurograms which are excited when I think about the future. "My neurogram of my self, including my self-regarding sentiment with all its powerful affects, is always active during my waking moments" ((7), p. 147).

11. *Psychological Dynamics* ((5), p. 59) = "psychodynamics" ((7), p. 86). "How one state of mind gives place to another....The flow of consciousness. To ascertain the causes determining this flow is the ultimate aim of psychology" ((7), p. 86).

The life space or parts of it often suffer strong and sudden changes ((5), p. 60). *One can represent the order of togetherness of the facts which exist for psychology only if one realizes from the beginning that the determination of spatial relations has to be based upon psychological processes and depends therefore on the nature and the laws of psychological dynamics* ((5), p. 63) = "Our problem is to formulate laws according to which our thinking proceeds" ((7), p. 86). "The system of brain paths followed by the excitement, together with its intensity at various parts of the system, ...define the thought at any moment" ((7), p. 87).

That the spatial structure of the life space depends on psychological dynamics itself shows the importance of the dynamical concepts. These are such concepts as cause of change, tendency, resistance, solidity, equilibrium, force, tension, etc. ((5), p. 63) = How neurograms and systems of neurograms "are formed and organized" ((7), p. 87) is seen in psychodynamics. Psychodynamics studies the causes determining the flow of excitement or nervous impulses through the neurones, forming them into neurograms, linking the neurograms together by lowering the thresholds or resistances between them, deepening or canalizing them, connecting them with the neurograms of affects (emotions and conations) and so increasing their driving power, or influence upon the flow of excitement and thus upon thought and conduct.

12. *The concept of connectedness, for instance the distinction between separate and connected regions, the distinction between different groupings of regions, is...of prime importance for characterizing both the person and the psychological environment. Changes of connexion, especially the uniting and separating of regions, are just as important for the psychology of perception as for the psychology of intention, satisfaction or friendship* ((5), p. 54) = "The physiological basis of all these"—the formation of neural habits, the association of ideas, memory, purpose ((7), pp. 154–68), "satisfaction" (pp. 244, 246), "comradeship or friendship" (pp. 285–86)—"consists in systems of nervous arcs of low resistance: arcs, that is, with thresholds[1] of various degrees of lowness" (p. 52). These systems of arcs, or neurograms, may extend widely in the central nervous system. They are by no means confined to the roof-brain. A simple case of remembering may involve my emotions as well as my perceptions. For instance, I am on a visit to a university town and happen to see on the pavement a familiar face and figure. I can't recall the name, or where I last saw the man. But I am perfectly aware of my emotional attitude towards him: I respect and like him. I say to myself "It must be so-and-so" (naming the Master of a certain college in the university, a man with whom I recently discussed some problems of secondary education); for, although I cannot be sure of any of his features, I know quite well that he shares my interest in colleges and schools. Next day, I suddenly realize who he was. Not the man I imagined, but the head of a college in another university and a governor of a school I know well.

13. *The topological concept of 'path' that connects two points...plays a fundamental role in the constitution of psychological spaces* ((5), p. 54). Neural paths of low resistance play an equally fundamental role in neurographics. For example, if I have a problem to solve "I can only achieve my purpose by finding an indirect path from A", my starting point, "to Z", my goal, "and so avoiding U", a barrier on the direct path from A to Z ((7), p. 171). (See lower figure on p. 28 below.)

The space of free movement of a person [is] a connected region ((5), p. 100). Inability to move freely where there are no neurographic connexions is illustrated in Figure 5 ((7), p. 170). (See below, p. 28.)

14. *Tension is...a state of a region relative to that of another region*

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[1] "When the intensity of excitement arriving at a synapse between two neurones is just sufficient to cause an impulse to pass along the second neurone, we...call this intensity the 'threshold' of the synapse" ((7), p. 35).

and it involves certain forces at the boundaries of the region ((5), p. 175) = Excitement is a state of a neurogram or of a system of neurograms. Its presence involves a tendency to spread or diffuse across the boundaries of the excited neurogram or system. "Excitement in any nervous arc tends to spread to every other arc that is connected to the first by synapses with thresholds low enough for that excitement to overcome. This law of diffusion is our second law of thought" ((7), p. 92). But "the extent to which the excitement spreads depends upon the relation between the intensity of the excitement and the thresholds of the synapses; and this varies between wide limits. If the brain is fatigued the synaptic resistances are high and the diffusion slight. The tired thinker finds that his ideas are lacking in wealth of meaning, his vocabulary is restricted, and his imagery poor" ((7), pp. 92, 93).

15. *Dynamic 'walls'.... A high degree of communication corresponds to a weak wall* ((5), p. 126) = A high degree of connectedness corresponds to weak resistance or low thresholds. A high degree of connectedness between two systems of neurograms—my neurograms for General Allenby's campaign in Palestine and for General Wavell's campaign in North Africa, for example, since I have lately read Wavell's book on Allenby—implies the existence of weak resistance to the diffusion of excitement from one system to the other, or a 'weak wall' between them. The excitement of one system, *A*, will therefore tend to spread to the other, *W*. In this way the state of the one influences the state of the other. Lewin writes: *By degree of communication of a region* [*A*] *with a region* [*W*] *we understand the degree of the influence of the state of* [*A*] *on* [*W*] ((5), p. 126).

Moreover, as Lewin observes ((5), p. 127) and as the law of forward conduction in nervous arcs might lead us to expect, the degree of communication, or weakness of resistance, may be greater in one direction than in the other. Since Wavell has written almost all I know of Allenby and since I know and am deeply stirred by other work which has lately kept Wavell very busy, my thought is much more likely to pass from Allenby to Wavell than from Wavell to Allenby.

Dynamic walls which may suffice to separate part systems to a high degree, if the forces in question are small, can become relatively unimportant in the face of the strong forces which arise during a state of high tension ((5), p. 189) = Thresholds which prevent the spread of excitement when the degree of excitement is low, are relatively unimportant when the excitement is high ((7), p. 93). "If the excitability is normal but the excitement intense, the nervous impulses may be diffused more widely than usual, so that thoughts may become unusually rich in meaning, and capable of expression in uncommon language" ((7), p. 93).

Dynamic Dependency.... The dynamic interdependency of two regions implies that the state of the one is influenced by the state of the other. It coincides therefore with the concept of dynamic communication ((6), pp. 168, 169) = Two interconnected neurograms—i.e. two neurograms separated by low thresholds—will tend to be excited together or in quick succession because, according to the law of diffusion (see no. 14 above), excitement tends to spread from one to the other. Thus the state of dynamic potency of one, or its power to influence the flow of thought according to the law of convergence (see no. 16 below), depends upon the state of the other.

16. *The Relative Weight of Situations* ((5), p. 137) = The relative intensity of excitement of systems of neurograms ((7), p. 104).

A child may be eating and at the same time listening to the song of a bird. The listening can be the major and the eating the minor activity or the reverse. Between two extremes many transitions are possible. Such cases of doing two things at once *are of common occurrence. But they offer considerable difficulty for description as well as for treatment of their dynamic facts* ((5), p. 138). A number of such cases have been described and treated neurographically ((7), pp. 111–15). Take for instance, Herbart's account of schoolboys whose minds are wandering until the teacher begins to tell a story. "No sooner did the words awaken old thoughts, forming strongly connected series with which the new impression easily combined, than out of new and old together a total interest resulted which drove the vagrant ideas below the threshold of consciousness, and brought... settled attention into their place (11)" ((7), p. 109). Lewin writes: *One can say in such cases that the person... is in two regions at the same time. Each of these regions is usually well structured and has the character of a situation.... If two or more situations overlap in such a way, each situation possesses at a particular moment a certain 'relative weight' (importance, potency) for the person. The change of this relative weight is a dynamically important process* ((5), p. 138).

To return to Herbart's account of the idle schoolboys. The "instruction being uninteresting, and discipline relaxed, a buzzing murmur was always to be heard, which invariably stopped for as long a time as an anecdote lasted. How could the boys, since they seemed to hear nothing, notice when the anecdote began?" ((7), p. 112). Neurographics gives the answer. According to its third law of thought, the law of convergence, "of all neurograms that include arcs of the higher level, the most excited tends to drain the excitement from the others" ((7), p. 104). And, since "every interest-system maintains its activity for some time after being excited", it follows that "when once any considerable interest-system—especially if it be rich in emotional elements—has been stimulated, it will tend for some time to reinforce the excitement of any neurograms connected with it, and so to make the excitement of other higher level arcs drain through them. An [active] interest thus tends to bring into the focus of consciousness whatever thoughts have any connexion with it" ((7), p. 108). Thus the process plays an important part in guiding thought.

Moreover, a corollary from the third law is that "of all active neurograms involving higher level arcs, that which is connected with (or forms part of) the widest and deepest interest-system tends, other things being equal, to drain the excitement from the rest" ((7), p. 110). And therefore, in the case of Herbart's schoolboys, "so soon as the neurograms excited (although very slightly, so that the corresponding thoughts never got beyond the fringe of an inattentive boy's consciousness) by the teacher's talk began to have connexions with an interest-system, although inactive, these neurograms tended to drain the impulse from all the others then active. Thus the corresponding thoughts came into the focus of consciousness. The interest-system was at the same time rendered active and maintained the boys' attention so long as it kept on being re-excited by the anecdote" ((7), pp. 112, 113). Here is an illustration of what Lewin calls a change in the relative weight of two situations: the teacher's talk, as he launches into an anecdote, becoming suddenly of *greater 'relative weight' (importance, potency)* in the minds of the boys than the competing thought. But neurographics is not content to observe that *the change of this relative weight is a dynamically important process*: it seeks to discover the process itself.

Again, Lewin observes that *Psychological regions of the environment can show very different dynamical properties* ((5), p. 115). *The fact that certain regions in the psychological environment* (like the teacher's talk in Herbart's experiment) *and within the person* (like the boys' interest in the subject of the anecdote) *influence other regions, both of the environment and of the person, may be taken as a criterion for connectedness in the topological sense* ((5), p. 54).

17. *The forces which result from a situation seem—other things being equal—to increase and decrease with the relative weight of this situation* ((5), p. 138) = The influence of an active system of neurograms, such as that corresponding to the boys' interest in the topic of the teacher's story, upon the flow of excitement, and so upon the person's thought and action, increases and decreases with the relative intensity of the

excitement of that system. This is a direct consequence of the law of convergence ((7), p. 104) quoted in the preceding paragraph.

18. *Dynamic unity of the whole person* ((5), p. 184) = The extent to which the self-system of neurograms is integrated, or interconnected, or free from high thresholds between its parts. To a perfectly integrated mind would correspond a single wide interest-system ((7), p. 236).

19. *Whole regions of the same degree of differentiation show a stronger dynamic unity if their part regions are less separated from one another* ((5), p. 184) = Other things being equal, a system of neurograms (the self-system, for example) exercises a stronger influence upon the person's thought and action if it is highly integrated, i.e. if its constituent neurograms are less separated from one another, or separated by lower thresholds.

This follows from neurographic laws. For, if the corresponding thresholds are lower in system A than in the otherwise similar system B, then the excitement of any part of A spreads (according to the law of diffusion) more easily to the whole of A than equal excitement in the corresponding part of B would spread to the whole of B; and therefore (according to the law of convergence) A will tend to be more dynamically potent that B, or to exercise more influence on the flow of excitement and so on the person's thought and behaviour.

20. *Unsolved...problems* ((5), p. 131). *We shall call boundaries (boundary zones) which offer resistance to psychological locomotion 'barriers'* ((5), p. 124). "A typical case occurs when the fulfilment of a purpose is obstructed by sense-impressions, whether from inside or outside the body" ((7), p. 169).

We shall speak of barriers of different strength according to the degree of their resistance ((5), p. 124). *If one wants to overcome a barrier, one usually does it not by continuing the original kind of locomotion with increased efforts but by choosing another kind of locomotion against which the barrier is weaker. The problem which the barrier sets is essentially one of finding the most suitable kind of locomotion or communication* ((5), p. 130). "Suppose, for example, that I wish to unscrew a metal lid from a glass jar. My first thought is of the jar with the lid stuck fast. Its neurogram is represented by A" (see lower figure overleaf from (7), p. 170, but first published in 1921). "My purpose is to grip the jar tightly with one hand and the lid with the other, and to twist one hand round the other without my fingers slipping. To this complicated movement corresponds the excitement of Z. When the movement takes place and Z drains the excitement the lid is unscrewed. But if the lid is stuck very fast a great

EXPERIMENTALLY INDUCED VISUAL PARAMNESIAS

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From the Cambridge Psychological Laboratory

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I. INTRODUCTION

THIS paper is concerned with a class of experiences to which we shall give the title *restricted paramnesias*. These experiences, commonly described as feelings or impressions of familiarity, are associated with a local failure of recognition. They are provoked by, and refer to, some more or less well-defined aspect of a perceptual situation. This aspect is always referred to the past history of the individual, but it cannot be endowed with an adequate localization in place or time.

We wish to distinguish restricted paramnesia from so-called *déjà vu*. In *déjà vu* there is always present the illusion of reliving a situation in its entirety, and the illusion is not obviously initiated by an isolated feature and always bears upon the situation as a whole. *Déjà vu*, moreover, may relate to a situation lacking all parallel in the history of the subject. Paramnesia, on the other hand, in its localized forms, almost always turns out to have a genuine foundation in past experience.

It is often argued that these two types of paramnesia can be ascribed to a common origin. MacCurdy (6), for example, claims, first, that every *déjà vu* has its source in a restricted paramnesia, and secondly, that it represents a mode of rationalizing this paramnesia. There is a certain amount of evidence in favour of this view to which we shall return at a later stage. For the present, however, we wish only to emphasize that the experiments to be described below are primarily concerned with paramnesia in its localized aspects. Their relation to classical *déjà vu* is conjectural.

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Our purpose in this paper is to demonstrate that *restricted paramnesias can be elicited under controlled experimental conditions.*[1] We argued that if, as appears likely, the so-called experience of familiarity depends upon some check to recognition in its cognitive aspects, then the introduction of an artificial resistance should suffice to provoke the phenomenon. If a subject is artificially prevented from recalling the conditions of an earlier perception, might he not experience a paramnesia if this perception is repeated?

Our experimental procedure was briefly as follows: Material, both visual and olfactory, was presented to the subject in the ordinary waking state. On the following day he was hypnotized and other material of the same kind presented under otherwise comparable conditions. At the same time, we endeavoured to induce a selective post-hypnotic amnesia for all the material given under hypnosis. On the third day of the experiment, the subject was tested for recognition in the waking state. The material given comprised all the items given on the two preceding days together with some completely new control material. Responses given to all material on this session were recorded *verbatim.*

Our procedure set up three distinct expectations in regard to the material shown under hypnosis: (1) That it would be treated in exactly the same way as the material shown at the first, waking, session, thus implying breakdown or failure of the post-hypnotic suggestion. (2) That it would call forth responses identical in character with those given to the control material. (3) That it would call forth responses having the general character of restricted paramnesias. The second expectation, if fulfilled, would signify an absolute failure of recognition for the material[2] of the hypnotic session. The third expectation, if fulfilled, would imply only a relative failure of recognition and might throw light on the origin of paramnesia in daily life.

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[1] This finding has been reported briefly in an earlier communication (2). It has also furnished the theme of a paper read at a meeting of the Bedford College Psychological Society in Cambridge on 22 November 1939 and at a meeting of the Scottish Branch of the British Psychological Society in Edinburgh on 1 February 1941.

[2] The expectation of an absolute as opposed to a relative failure in recognition is strengthened by Strickler's results on post-hypnotic amnesia (8). This author found that visual characters learned during hypnosis were not recognized by the subject on awakening from the trance. In discussing this experiment, Hull writes that "the subjects invariably said that they had never seen the characters before, thus showing a perfect symbolic amnesia" ((4), p. 134). Claparède (3) reports analogous findings.

We shall consider in this paper only the visual material and the responses to which it gave rise. Olfactory paramnesias will be discussed in a later paper.

II. EXPERIMENTAL METHOD

(1) *Material*

The material comprised twenty-four picture postcards selected more or less at random. With very few exceptions, none of the pictures was at all generally known. The cards were divided into three sets of which the first two comprised six cards each and the third twelve cards.

The first set, called *Series A*, was always given to the subject on the first day of the experiment in the waking state. The second set, *Series B*, was given on the following day under hypnosis. The third set, *Series C*, was given together with the other two series on the third day as a part of the recognition test.

Series A. This series comprised the following six cards:

A 1. *Belgian Church:* an uncoloured photograph of a little-known church.

A 2. *Three Flints:* an uncoloured photograph of three prehistoric flints.

A 3. *Dutch Women:* a coloured photograph of a group of women in national dress.

A 4. *Still Life:* a coloured reproduction of a little-known oil-painting. A flask of wine and some brightly coloured flowers were prominent.

A 5. *Venetian Scene:* another brightly coloured reproduction.

A 6. *Still Life:* a coloured reproduction of an obscure oil-painting showing two game-birds, a blue tankard, and other objects.

Series B. This comprised the following six cards:

B 1. *Japanese Print:* a coloured reproduction of an obscure print. It showed a girl in Japanese costume, a fan, a lantern, and some peacock feathers.

B 2. *Nine Flints:* an uncoloured photograph of nine prehistoric flints.

B 3. *Still Life:* another coloured copy of an obscure still-life in oils. A number of game-birds, a game-bag and a gun were prominent in the picture.

B 4. *Church Interior:* a coloured reproduction with a peculiar reddish hue.

B 5. *Egyptian Girl:* a coloured photograph of an Egyptian girl with an earthenware pitcher on her shoulder.

B 6. *Reynolds's "Marquess of Crewe":* a reproduction in colour of the celebrated picture. This was the only picture in the series likely to be at all widely known.

Series C. This series comprised twelve cards:

C 1. *"Coming Through the Rye":* an illustration of the popular theme.

C 2. *Mulready's "The Sonnet":* a coloured reproduction.

C 3. *Church:* a plain reproduction of a little-known German church.

C 4. *Junk:* a Chinese water-colour.

C 5. *Church Interior:* a coloured reproduction of an unnamed church.

C 6. *Three Flints:* an uncoloured photograph similar to A 2.

C 7. *Dutch Peasants:* a coloured photograph.

C 8. *Still Life:* a copy in colour of a little-known oil-painting. A stag suspended by the legs was the most prominent feature.

C 9. *Still Life:* a coloured copy of a picture showing fruit and leaves.

C 10. *Marseilles Harbour:* a view of the town and harbour.

C 11. *Nine Flints:* a plain photograph of nine prehistoric flints strongly suggesting B 2.

C 12. *Rottingdean School:* a plain photograph of the school library.

A comment on the *degree of similarity* between the cards in the various groups is in place:

(1) *Still Life.* All these cards were made by the same firm and had a certain generic resemblance. A certain thread of similarity ran through the pair A 4 and C 9 on the one hand, and through the group A 6, B 3, and C 8 on the other. But there was a great deal of difference, both in arrangement and detail, between the individual cards in both groups.

(2) *Prehistoric Flints.* The four cards A 2, B 2, C 6 and C 11 all depicted prehistoric flints of the same general type. The resemblance between A 2 and B 6 (Three Flints) on the one hand, and between B 2 and C 11 (Nine Flints), on the other, was extremely marked. Either of these pairs could fittingly have been used in a test of normal differential recognition.

(3) *Church Interiors.* There was some slight similarity of content between the cards B 4 and C 5 (Church Interiors). This was not sufficiently marked to lead one to expect any confusion on an ordinary test of recognition.

The *order of presentation* of the cards on the recognition test was always as follows: (1) C 1, (2) C 2, (3) A 3, (4) C 3, (5) B 3, (6) C 4, (7) A 1, (8) C 5, (9) B 1, (10) C 6, (11) A 6, (12) A 5, (13) C 7, (14) C 8, (15) B 2, (16) B 4, (17) C 9, (18) A 2, (19) C 10, (20) B 5, (21) A 4, (22) C 11, (23) B 6, (24) C 12.

(2) *Subjects*

Five subjects (W, P, E, Y, and M) took part in the investigation. Four of these were undergraduates, the fifth a young army officer. Their average age was 22 years. All five had been hypnotized once previously by one of us (H. B.).

The results given by two of the five subjects will be presented in full. In the remaining three cases the amnesia was less complete and the results of much less interest. For purposes of comparison, however, these results are outlined in § V.

(3) *Procedure*

A brief outline of the procedure has already been given. It will be remembered that three separate sessions were demanded in each case.

These we may call *Waking Session I*, *Hypnotic Session*, and *Waking Session II* respectively. The procedure on each session was as follows:

Waking Session I. The subject was first told about the object of the experiment. This, we said, was to investigate the relation between susceptibility to hypnosis and 'suggestibility' in perception. The problem was intentionally stated in this vague way in order to minimize the connexion between the waking sessions and the events of hypnosis. No clue was given as to the true object of the experiment. Usually an abbreviated Rorschach test was introduced on the first session to point our explanation.

Exposure of the cards was undertaken without special apparatus. The instructions were as follows: "This is an experiment on perceiving. I am going to show you a number of cards and I want you to look carefully at each card that I show. Each card will be exposed for 30 sec." The cards were then placed successively on the desk in front of the subject for the stipulated time interval. Comments were not recorded, but at the end of the series the subject was asked whether he had seen any of the cards before.

Presentation of the test odours followed immediately. The method will be described later in connexion with that part of the experiment.

Hypnotic Session. This session was usually conducted in another room. In the case of Subject M, however, all three sessions took place in the same room in order to control for possible differences of locality. The subject was hypnotized by one of us (H. B.), the other keeping a careful record of the procedure. Hypnosis was induced in the ordinary manner (see, e.g. Banister(1)), and generally carried to a stage of moderate depth. We agreed that the depth of hypnosis was greatest in the case of W, moderate in the case of P and M, and comparatively light in the other two subjects. The details of procedure varied slightly from case to case, but the text of the instructions was always adhered to rigidly.

Exposure of the cards was prefaced by the following instructions: "I have here a number of cards. I am going to show them to you one by one. I want you to look at each card very carefully. You have half a minute in which to look at each card." The subject was then told to open his eyes and was given the first card to scrutinize. On its removal after 30 sec. he was told to close his eyes and given the following suggestion: "After you wake up, you will have forgotten all about this card, and you will *not* be able to recognize it if it is shown to you to-morrow." The subject was then given the second card and told to look at it very carefully. On its removal, the above suggestion was repeated. The remaining four cards were then given in the same way, the suggestion that he would forget being repeated in exactly the same words each time. At the end of the series, a more generalized reinforcing suggestion was given: "When you wake up you will have forgotten all about these cards, and you will *not* be able to recognize any of them should they be shown to you to-morrow."

The method of presenting the odours will be described elsewhere. But a further generalized suggestion was repeated at the end of their exposure: "After you wake up, you will have forgotten all about the cards and also all about the odours. You will forget all about them. You will not be able to recognize the odours and you will not be able to remember their names. You will not be able to recognize the cards. You will have forgotten all about the cards and all about the odours, and you will not be able to remember that I presented them to you."

The subject was generally awakened shortly after the above suggestion was given. Precautions were always taken to remove all trace of the odours before the subject was awakened.

Waking Session II. The subject was tested for recognition approximately 24 hr. after the hypnotic session. He was told that the present session was a continuation of that begun two days previously. It was again emphasized that we were interested merely in visual and olfactory perception.

The sequence of test odours was generally given first. After the responses to these had been recorded, the series of twenty-four cards was given as already described. The instructions on this test were as follows: "Now I am going to show you a series of cards. This is also a test of your capacity to describe your own experiences. Look at each card carefully, *and then tell me anything that comes into your mind in connexion with each card*: What it is like, what it suggests to you, whether you have seen it before, whether it appeals to you or not, and so forth. You have 20 sec. in which to look at each card, but you need not wait until the card is taken away before starting to tell me about it." The interval between exposure of the card and the first verbal response was recorded. Supplementary questions asked from time to time by the examiner were recorded verbatim together with the answers.

The three stages of the experiment were conducted by one or both of the present writers in every case but one. In this case, *each of the three sessions was conducted by a different person.* The subject was M. *Series A*, on the first day of the experiment, was presented by Dr K. J. W. Craik. The hypnotic session was conducted by the present writers as in the other cases. Dr J. M. Blackburn conducted *Waking Session II* and presented all the material for recognition. Dr Blackburn was unaware of the constitution of the test series. He had not been told which (if any) of the cards had previously been shown in the waking state, which had been given under hypnosis, and which were entirely new. This procedure was adopted to reduce to a minimum unwitting suggestion on the part of the experimenter.

III. RESULTS WITH SUBJECT W

Subject W was a third-year psychology student with previous training in chemistry and physics. He was hypnotized readily in a preliminary session, and found to show a considerable degree of spontaneous post-hypnotic amnesia for the trance.

We shall consider the responses given by this subject to the three sets of material, A, given in waking session I; B, given in hypnotic session; and C, given in waking session II in the test of recognition on the third day. It is convenient, however, to divide the material as a whole into two classes: (*a*) *Unique Cards*, and (*b*) *Similar Cards*. *Unique* cards are all those cards in any series wholly dissimilar to any other member of any other series. *Similar* cards are those bearing a fairly marked resemblance to some other card in one of the other sets. These two classes may be taken one at a time.

(*a*) *Unique Cards: Series C.* Seven of the twelve cards comprising this series may be treated as unique for present purposes. The subject's reactions to all seven were entirely in keeping with the normal reaction to unfamiliar material. We may quote two of these responses for comparison with the other groups.

C 1: "*Coming Through the Rye*". "I can't say I've seen that before. It's like 'Coming through the Rye'—Yes, I see it is that. It doesn't look like an English card."

C 4: *Junk*. "No, I havn't seen that. It's rather like some cards we've been using in the lab. Not the same, though. It also reminds me of the A.S.W. film 'The River'."

These two responses are entirely characteristic of the normal treatment of comparatively unfamiliar material.

Series A. Three of the six cards in this series were treated as unique. All three were instantly recognized and referred to the session on which they had been shown. We may quote one of these responses:

A 3: *Dutch Women*. "I've seen that before. I saw it on Monday (the first day of the experiment). I remembered it by the form of the main figures and the baskets; and on Monday I associated it with a Flemish picture."

The recognition in this case is prompt, decisive and circumstantial. There is some recall of the earlier reaction made to this card.

Series B. Three of the six hypnotic session cards can be classed as unique. These are B 1, B 5, and B 6.

B 1: *Japanese Print*. "I've seen that before. On some exotic sort of Christmas card or Japanese lantern. I remember the terrific 'eye' in the peacock's feather. I'm trying to connect it up with several things: First, the figure. Secondly, the Japanese lantern. And thirdly, the peacock's feather. I don't know—I think I associate the lantern with a Christmas card and the figure probably with productions of the *Mikado* which I've seen. But they don't fit in one whole. The peacock's feather fits in as an essential part of the thing, but doesn't seem to fit in with the rest. I can find satisfactory explanations for the other two things but not for the peacock's feather. But I think I've seen them as a whole."

This response is decidedly interesting. In the first place, there is an immediate judgement of reference to the past. In the second place, the subject adduces incorrect evidence in support of this judgement. In the third place, his rationalizations follow three entirely different lines, each of which is initiated by a different feature of the original card. These are never entirely harmonized to the subject's complete satisfaction.

A few additional questions were given in this case. These may be recorded together with the corresponding answers:

What was your experience on exposure of the card when you said: "I've seen that before"?—"A feeling that I'd seen it before—then that I hadn't but had seen parts. It's not one of the one's I saw on Monday." *Had the experience anything in common* *with those cases in which you thought you had smelt an odour before but couldn't remember exactly where or when?*—"To a certain extent. With the odours it seemed I had smelt them before but couldn't place the time. With this I couldn't think of any possibility where I could have seen it before. I think I must have looked at it before, but can't think of any time when I would be looking at something like that."

These additional responses show that the subject is prepared to treat his original experience on exposure of the card as a 'feeling'. He does not, however, develop in any detail the status of the response.

The above reaction taken as a whole may be described as an *experimental paramnesia*. It is marked by an obvious element of fabulation.

B 5: Egyptian Girl. "I think I've seen that. In the *National Geographical Magazine* of America or an Egyptian State Railways poster—the colouring is not bright enough for that. It may have been in the *National Geographical Magazine.*" *You think you've seen that before?*—"Yes. It's not one I saw on Monday. I think it's the sort of thing the *National Geographical Magazine* produces. I can't remember reading an article on it, though. I think it must have been in there." *Can you describe your experience at all on presentation of that card?*—"That I'd seen it before. There were two possibilities: First, the poster; secondly, the *National Geographical Magazine.* It's about a couple of years since I read any of their stuff, but I remember their colour photographs—pretty vivid." *You do think you've seen it before?*—"I'm pretty sure I've seen it before." *Would you describe that as a feeling of familiarity?*—"Yes—but not for long. I think I've traced it down fairly completely. I think I've got it. It reminded me of this magazine and the things that have been in there. That knocks out the feeling of familiarity pretty well."

Again we note that the subject at once relates the picture to an acceptable context in his past experience. But this reference again lacks precise localization in place or time. Again, too, rationalization of a wholly constructive character supervenes on the original judgement. We note also that the subject is prepared to accept the expression 'feeling of familiarity' as an adequate description of his experience. He stipulates, however, that this is dispelled as soon as an adequate explanation is forthcoming. Comparable reports are often obtained in descriptions of paramnesia in everyday life.

B 6: Reynolds's "Marquess of Crewe". "Yes, I think so, although I'm probably confusing it with Gainsborough's 'Blue Boy'. It's the style more than anything else I remember." *Can you tell me where or when you saw that?*—"No, I don't think I've actually seen that card: It's similar to Gainsborough's 'Blue Boy'."

This response is harder to interpret. It is noteworthy that the card is not itself recognized, and that the familiarity it elicits is displaced on to a somewhat similar picture. Doubtless the subject was genuinely acquainted with the latter. Perhaps we may speak of *displacement by analogy* as governing the rationalization in this case.

(*b*) *Similar cards.* The similar cards fall into four well-defined groups which may be briefly described:

(1) *Still Life* (α). Three cards, B 3, A 6, and C 8 were all copies of oil-paintings showing dead game and the like.

(2) *Still Life* (β). Two cards, A 4 and C 9, in which fruit and flowers were prominent.

(3) *Prehistoric Flints.* Two groups of two pairs: (α) A 2 and C 6 (Three Flints); (β) B 2 and C 11 (Nine Flints). The resemblance between the two members of each pair was extremely close.

(4) *Church Interiors.* Cards B 4 and C 5 respectively.

We may consider the responses to the members of each of these four groups in their order of presentation on the recognition test.

(1) *Still Life* (α).

B 3: Still Life. "I've seen that before. Well, I don't know. I associate it with an old pub. I seem to remember an old blue tankard with a lid. I think it was on a bench. The name 'Oilette' (trade mark) I remember. I don't know: it's probably one of the same series I'm confusing it with."

A 6: Still Life. "That's the one I saw on Monday. I don't know. There isn't a gun in it. But there is the shelf and the tankard. I seem to have in mind a composite of the two."

C 8: Still Life. "No I haven't...I definitely haven't seen that one. I would have remembered the young deer. I definitely haven't seen that."

It will be seen that the control card, C 8, is definitely rejected. But there is considerable confusion and uncertainty in the other two cases. In the case of the hypnotic session card B 3, the subject is evidently perplexed by the absence of the blue tankard in fact present in the waking session card A 6. In the case of the latter card, moreover, he is equally perplexed by the absence of a gun which had in fact appeared in the card seen when under hypnosis. The position is instructive for two reasons: First, it shows that a hypnotic inhibition can interfere with response to a *waking* session card. Secondly, it suggests that the paramnesia evoked by the hypnotic inhibition is displaced by projection on to a member of the waking series. The whole process here involves both condensation and inter-serial displacement.

(2) *Still Life* (β). Card C 9 was shown as the 17th, and card A 4 as the 21st member of the recognition sequence. There was no confusion of any kind between these two cards. And yet the degree of similarity was no less marked than in the preceding couple.

C 9: Still Life. "No, I've never seen that before—nor anything like it. It's the sort of thing I would have remembered as it's got rather exotic colouring."

A 6: Still Life. "Yes. That's one of the one's I saw on Monday or Tuesday or whenever it was (the first session). I remember it distinctly by its being laid out on the board—and the straw—and the peculiar poppy or whatever it is."

These two responses provide an excellent illustration of normal differential response to visual material. They contrast sharply with the responses to the preceding pair.

(3) *Prehistoric Flints:* (α) *Three Flints;* (β) *Nine Flints.* We shall quote the reactions to these four cards in their order of sequence on the recognition test.

C 6: *Three Flints.* "That's one I saw on Monday, I think. Not sure....I think the one I saw on Monday had more pebbles in it. It doesn't seem to 'fit', quite."

B 2: *Nine Flints.* "I think that's the one I saw on Monday. That's the one I saw on Monday. I remember counting up to nine. Those (flints) fit better with my naming."

A 2: *Three Flints.* "Oh I've seen that before! I remember the 'wolf's head' sort of thing (outline of one of the flints). Rather like the one I saw on Monday, but I remember counting up to nine. I seem to remember the 'chisel-point' (another flint) on the left. I remember that."

C 11: *Nine Flints.* "No, I haven't seen that one before, although it has got nine. They weren't arranged in that order."

These four responses must necessarily be considered together. The first point to note is the unqualified rejection of one of the new cards, namely, C 11. The second outstanding point is the pronounced confusion between the remaining three cards. The first to be shown, a control card, is at first accepted as having been seen previously, but is later rejected on the ground that the card actually shown had more flints in it. The second, the hypnotic session card, is accepted, *but erroneously referred to the first waking session*. The third card is also identified, but the subject apparently persists with his assertion that the card shown on the first session had nine flints. Such a card was shown during the hypnotic session. Thus the waking session card, though identified correctly, is not referred to its appropriate session whereas the hypnotic session card is rationalized by inter-serial displacement.

(4) *Church Interiors.* This was a case in which a hypnotic session card bore a certain similarity to a control card. The latter was at once rejected, but the former initiated a beautiful paramnesia:

B 4: *Church Interior.* "I've seen that one before. I don't know when and I can't think where I could have been to see it. The peculiar colour of the wood or whatever it is....Normally I only see postcards of cathedrals when I'm at the cathedral concerned, but I can't remember anything like that. Yet the red colour sticks out very definitely." *How would you describe your experience on your first reaction to that card?*—"Rather surprised: I thought it might have been one I saw on Monday, yet the only one connected with a church was one we've had before. Yet I feel fairly sure I've seen it." *On what do you base your verdict?*—"The colour of the card. To say the least it's not normal. It seems that I've seen it before. It may be another type of card with similar colouring." *Have you ever had any experiences in ordinary life similar to that?*—

which he has himself raised. Probably a few carefully chosen questions would have elicited some interesting reactions. The experimenter, however, was exercising great care in order to avoid influencing the subject by suggestion in any way.

B 6: *Reynolds's "Marquess of Crewe"*. "I have seen a picture like that before. Probably in the National Gallery. It's rather grotesque seeing a boy dressed up like that...."

We are unable to interpret the significance of this response. Certainly the picture is not related to the period of hypnosis, but what the subject has to say about it in other respects may well be true. His behaviour is very similar to that of the previous subject.

These three responses lack the elaborate justifications which were found to be so characteristic of Subject W's behaviour when confronted with the unique hypnotic session material. This difference may well depend upon some temperamental factor. In other respects, however, the paramnesias set up are very similar in the two cases.

(*b*) *Similar Cards*. The four groups of similar cards have been described above (p. 38). Our presentation of the results follows that described in connexion with Subject W.

(1) *Still Life* (α).

B 3: *Still Life*. "I have the impression...I have the impression that I have seen that before..." (*pause*). *Do you remember where or anything about it?*—"I have a feeling I should like to tell you, but why can't I say it? I have the same feeling as I had....I know exactly....There is something in my mind which is making continual efforts to tell you but my tongue won't get on with the job. Now where did I? I am just saying to myself now: 'Come on, don't be so dumb! You know you have seen it before!'...It's like making up one's mind to jump over one of those walls that give way under you—those dry walls in Yorkshire—I simply can't give you the answer. It's a very tiring process too."

This is a beautiful example of inhibited recognition. The subject is confident that he has seen the card before, and confident, too, that he knows all about the earlier occasion. Yet he is absolutely unable to formulate, or at least to communicate, the necessary information. It is interesting, too, that M shows considerable insight into his perplexity, and does not attempt to rationalize it in the rather facile manner which we have noted in connexion with the previous subject, W.

A 6: *Still Life*. "I saw this on Monday (the first day of the experiment). There is the old calendar or almanac probably giving remedies for all sorts of ills...."

We note that this waking picture is at once identified and ascribed to the session on which it had been shown.

C 8: *Still Life.* "I have not seen this before. I suppose this fellow was killed for food...probably it is the dull colour of the picture but I don't like it...."

It is interesting to compare the response to B 3 with those given to the other two similar cards. The first is a typical paramnesia of recognition; the other two are typical differential responses to visual material.

(2) *Still Life* (β). There was no trace of confusion between C 9, one of the control cards, and waking session card A 6, despite their similarity in content. The first was treated as an altogether new stimulus; the latter was accepted with the characteristic response: "Yes, I have seen this on Monday."

(3) *Prehistoric Flints:* (α) *Three Flints;* (β) *Nine Flints.* We may quote these responses in their order of sequence:

C 6: *Three Flints.* "That reminds me of a card I saw on Monday. But I am sure it was not the card I saw on Monday because these flints are of a much better shape. They give the impression of being hand-made, etc...."

B 2: *Nine Flints.* "I didn't see that card on Monday but I have seen it somewhere. The flints are not very well-shaped, etc...."

A 2: *Three Flints.* "I saw this on Monday. Here they are—this flint and chalky stone and the central one is so smooth...."

C 11: *Nine Flints.* "I may have seen this before. I...For some reason that picture irritates and worries me because it makes me think that my memory is not so good as it ought to be. If I have seen that picture before, why can't I say where I have seen it? Perhaps because I am no longer interested in those kinds of thing."

This subject experiences no difficulty in discriminating the cards with three flints. But the other pair, those with nine flints, give rise to the paramnesic type of reaction. Both are treated as having a parallel in past experience, but no precise details can be given in either case. If anything, the control card stimulates greater perplexity than the one seen when under hypnosis. The suggestion that the subject fears some deterioration of memory is particularly interesting.

(4) *Church Interiors.* Card C 5 was rejected without hesitation. Card B 4 gave rise to an interesting response probably in line with those which have been described in the other cases.

B 4. "I have a feeling of distaste for that—as though I had been thinking about it for some time....It is rather a gaudy church...."

The interesting point here is the affective reaction to this card, and the explanation which the subject offers for it. It suggests that unpleasant feeling tone may be associated with the hypnotic resistance.

We may now give a brief summary of the results from Subject M:

(1) All unique cards in the C series were treated as new, and all in

the A series as having been seen previously. Normal differential reactions were recorded in all cases.

(2) The three unique cards seen under hypnosis gave rise to recognition of the paramnesic type. In the first case, spontaneous reference was made to common descriptions of *déjà vu*. In the second case, the subject at once asked himself where he had seen the subject of the card before. In the third case, the familiarity initiated by the card appeared to be displaced on to a similar well-known picture.

(3) The hypnotic session member of a group of three similar cards set up an outstanding paramnesia. The other two members of the group were dealt with normally.

(4) In a second group of four similar cards, a new control card was discriminated successfully from a very similar card seen during waking session I before the latter had been exposed.

One seen when under hypnosis was, however, confused with its counterpart in the C series, and both elicited pronounced paramnesias.

(5) Subject W was not given to fabulation of a constructive and facile character. He rationalized to some extent from time to time. In the main, however, he appeared aware of some check in interpreting the test material. He did not adduce any connexion between this difficulty and the events of hypnosis.

V. RESULTS WITH OTHER SUBJECTS

Three other subjects took part in the experiment. In two cases (E and Y), the outcome was unsuccessful, the suggested amnesia did not take effect, and both subjects were able to recall the events of hypnosis.

Subject Y. This subject recognized five of the six *B* series cards and ascribed them correctly to the hypnotic session of the previous day. He incorrectly attributed one of the *A* cards to this session.

B 3: *Still Life*. "Yes, I've seen that before. I think I saw it yesterday."
B 5: *Egyptian Girl*. "Yes, I saw that yesterday."

This subject, when asked later about the events of hypnosis, said: "I can remember being shown some pictures; they seem immediately familiar to-day. I can remember about six pictures." Asked whether he had been given any special instruction in connexion with these pictures, he said: "Well, he (H. B.) said something about not remembering pictures, but when I woke up I couldn't get out of my head what the pictures were: it seemed rather incongruous at the time and I was a little worried about it."

Subject E. Three cards of the hypnotic session were definitely identified by this subject. One was identified but ascribed to the wrong session. Two were thought to have been shown under hypnosis, but the subject was far from certain about this. We may quote four of the responses to these six cards:

B 3: Still Life. "Aha! That was not shown to me by you on Monday but by Dr Banister last night...."

B 1: Japanese Print. "Um...not sure whether I've seen that one or not. It vaguely suggested one shown by Dr Banister last night."

B 2: Nine Flints. "First thing: Have I seen it before or not? May be one Dr Banister showed me. Then the name *six* came into my mind; then the Birmingham Museum."

B 4: Church Interior. "Um...registered that I've seen this before but I don't know where. Outside St John's Chapel....I think you showed it to me on Monday."

This subject told us later that he remembered practically everything about the hypnotic session, and had anticipated a recognition test on the third day of the experiment.

Subject P. The results in this case occupy an intermediate position between our two successes and our two failures. This subject was conscious during the recognition test of some vague recollection of having been shown some pictures on the previous day. On the other hand, he was completely amnesic for the odours.

He brought forward his recollection of hypnosis to justify recognition in two cases of response to hypnotic session material. In three cases, however, other more constructive interpretations were forthcoming. All the members of Series A were identified and all the twelve control cards rejected. We may quote his responses to the six hypnotic session cards:

B 3: Still Life. (Obvious perplexity.) "That's familiar...it's by the same person who drew the one with the two birds in it and the brass pot." *Did you say it was familiar?*—"I don't know. It's the same type of still life as the other one was."

B 1: Japanese Print. "I seem to have seen this one before....I remember the peacock's feather." (Very puzzled.) *Can you describe your experience on first seeing that?*—"I am pretty sure I had seen it—the peacock's feather. I don't know why." *I suppose you've no idea where you might have seen it before?* (Hesitatingly.) "I do remember Dr Banister showing some yesterday."

B 2: Nine Flints. "That's vaguely familiar too. I seem to remember the nine flints or whatever they are." *Anything else to say about that one?*—"No."

B 4: Church Interior. "Er...I think I remember that. I remember the two battle-flags and the church, and I remember I was struck by the electric bulbs." *Do you think you've seen that before then?*—"I'm pretty sure I have." *Can you tell me anything else about that one? Where you have seen it?*—"I don't think it was one of the six (A cards). It's similar to the one of the chapel, but I definitely hadn't seen that." *But you do think you have seen it before?*—"Yes, I do think I have."

B 5: Egyptian Girl. "I think I remember this one. It's the sort of thing one has seen at Tourist Bureaux." *Anything else about it?*—"No." *You think you've seen it before?*—"It seemed vaguely familiar. I may have seen something like it at Bell's (Tourist Agency)."

B 6: Reynolds's "Marquess of Crewe". "That...that one I seem to remember.... I remember seeing Henry VIII, and I remember the dog biting itself." *You think you've seen it before?*—"Yes, I'm pretty sure." *Where have you seen it, do you think?*—"I don't know, unless Dr Banister can have shown it to me. I can't place it really. I'm not as certain as with those you showed me (Series A)." *Do you remember having seen it before?*—"It's familiar, I think."

These responses are quite in line with those obtained from Subjects M and W, despite the suggestion made in two cases that the cards might have been shown under hypnosis. The inhibition, though less complete than in the first two cases, is nevertheless adequate to cause some perplexity and difficulty in recognition. One may notice the attempted serial displacement with B 3 and the rationalization by analogy with a travel poster in the case of B 5.

VI. QUANTITATIVE DATA

We have summarized in Table 1 the responses given to all three sets of cards by the five subjects on the third day. In col. 1 is given the number of cards in each set which provoked unequivocal judgements of recognition and reference to the correct session. These we may call cases of *normal recognition*. There are two instances of 'false' recognition due to control cards, which were similar to others which had been shown during waking session I, being twice treated as belonging to the A series. In col. 2 we show the total number of cases in which a card was referred to a session other than that on which it had been originally shown. This we have called *inter-serial displacement*. Entries under the heading A denote cards in this series which were displaced to the hypnotic session, and entries under the heading B denote hypnotic session cards displaced

Table 1

	Col. 1 Normal recognition			Col. 2 Inter-serial displacement			Col. 3 Rejection			Col. 4 Experimental paramnesias		
Subject	A	B	C	A	B	C	A	B	C	A	B	C
W	5	—	1*	—	1	—	—	—	11	1	5	—
M	6	—	—	—	—	—	—	1	11	—	5	1
P	6	2	—	—	—	—	—	—	12	—	4	—
Y	4	5	—	2	—	—	—	1	12	—	—	—
E	5	5	1*	—	1	—	1	—	11	—	—	—
Total	26	12	2*	2	2	—	1	2	57	1	14	1

* Incorrectly recognized.

to the waking session. Col. 3 shows the number of cards in each group which were judged as being seen for the first time. We call these *rejections*. Col. 4 gives the total number of cards referred to an unspecified or fabulated context. These we call *experimental paramnesias*.

The total number of recorded paramnesias is sixteen. This represents 13 % of the total responses given by all subjects on the recognition test. Of the total responses given to Series A and B together, 25 % are experimental paramnesias. Of the total responses given to cards shown during the hypnotic session alone, 47 % are paramnesias. If we consider only the responses given by Subjects W, M, and P this figures rises to 78 %.

The mean *reaction times* for Subjects W, M, and P, at the third session, to the A and B sets of cards are given in Table 2. Reaction time, for present purposes, was taken to mean the interval elapsing between presentation of a card and the first verbal response given by the subject.

Table 2

	Reaction times during waking session II to cards seen during			
	Waking session I (A)		Hypnotic session (B)	
Subject	Mean	S.D.	Mean	S.D.
W	2·3	1·1	4·2	2·2
M	7·0	8·3	26·7	12·4
P	2·5	1·8	4·3	1·5

It will be seen that all three subjects show a longer mean reaction time in dealing with the hypnotic session material. The scatter, however, is so large that no definite conclusions can be drawn. It is possible that a repetition of the experiment using more subjects would give a significant difference in reaction time to the cards shown in the waking and hypnotic sessions.

VII. DISCUSSION

Our results clearly show that it is possible to provoke, under experimental conditions, states of consciousness which appear to be the same as normal restricted paramnesia. The greater ease with which these phenomena were elicited in our first two cases is probably due to two main factors: First, the relative depth of hypnosis; second, individual susceptibility to post-hypnotic amnesia. Our first two subjects were hypnotized fairly deeply, and both appeared to be completely amnesic not only for the prohibitions given but also for the greater part of the trance. In the case of E and Y, on the other hand, hypnosis was comparatively light, and both subjects were afterwards able to remember a fair amount about the procedure. Both subjects could recall having been shown pictures and told to forget them. It was unfortunate that we were unable to use subjects who on other occasions had shown a marked degree of spontaneous amnesia for hypnotic experiences. We would suggest tentatively that a temperamental factor is involved in addition to mere degree or depth of hypnosis.

The significant point in our results is that paramnesias were so consistently obtained under conditions in which amnesia for the hypnotic session was complete. There were only two cases of out-and-out rejection of cards shown under hypnosis, and in one of these, at least, there was more than a suspicion of the paramnesic type of response. Our results consequently differ from those of Strickler (8) and Claparède (3), both of whom report absolute failure in recognizing items presented in the hypnotic state. Some possible reasons for this divergence at once suggest themselves. In the first place, both Strickler and Claparède used comparatively meaningless material. Our own material, on the other hand, was designed to appeal in some measure to the interests of our subjects. In the second place, these authors may well have induced more profound hypnosis than we did. It seems not altogether unexpected that disconnected and utterly artificial material previously given under deep hypnosis should fail to stimulate even a chord of recognition when given once again in the waking state. All the same, we believe that further work is necessary to account for this discrepancy between the earlier work and our own results.

The experimental paramnesias illustrated in our paper have three fairly well-defined moments: (*a*) an immediate act of reference of the stimulus to an unspecified context in the history of the individual; (*b*) a phase of perplexity, sometimes mildly distressing, and always associated with an effort to formulate this context in more definite terms; (*c*) an act of rationalization often wholly constructive in content. This phase was more prominent in some subjects than in others, and might be entirely lacking.

The judgement of reference was typically conveyed by the statement: "I've seen that before." This judgement almost always carried conviction in the subject's own mind. It is true that doubts might be expressed later on, but this first reaction was generally expressed with considerable confidence. In some cases the judgement appeared to be based on a primary affective reaction to the presented material. But the evidence certainly does not warrant our saying that this was so in every case.

Introspective testimony was seldom given unasked, and all the answers to our special questions are open to the suspicion of suggestive influence. We had extremely few reports of a clear-cut experience of familiarity anticipating the judgement of reference. But the lengthened reaction time in these cases possibly suggests that some such factor plays a part in governing the response.

The phase of perplexity was always associated with difficulty in localizing the previous perception of a card in time and place. Sometimes a rationalization followed so rapidly on the initial statement that this stage did not appear. In the great majority of cases, however, a notable interval elapsed between the first overt reaction and the final acceptance by the subject of the conditions under which he believed the earlier presentation to have taken place. In cases in which no explanation of any kind could be elaborated, the phase of perplexity persisted until the exposure of the next card in the series.

The phase of rationalization cannot be sharply distinguished from the preceding phase, which it always overlaps. The process, we believe, has a two-fold origin: First, it expresses the normal tendency to allot any stimulus initiating an impression of familiarity to an acceptable context in past experience. Secondly, it expresses the subject's attempt to comply with the post-hypnotic suggestion while evading total inability to deal with the presented material. In any concrete case, the process has always as its object the creation of an acceptable and more or less adequate explanation of the somewhat paradoxical familiarity of the material. It is possible, moreover, that a degree of conscious *critique* lower than the normal was in evidence under our special conditions.

We may now pass on to consider in brief the origin and psychology of experimental paramnesia. In the first place, it is important to consider how far our findings are a function of the instructions given under hypnosis. The subjects, it will be remembered, were simply told that they would forget each successive stimulus and be unable to recognize it on the following day. We provided no definition of the term "to recognize", and it was obviously left to the individual subject to interpret the command in the light of whatever idea he might have of its meaning. Now popular connotations of the term 'recognition' are extraordinarily varied. Many people do not habitually include under the term mere impressions of familiarity with vague localization of the stimulus in past experience. It follows, therefore, that our subjects may have understood the suggestions as excluding the possibility of detailed recognition alone. If this were so, the paramnesias would represent mainly those aspects of the recognition process which had not been made subject to an artificial repression. This ambiguity might have been avoided either by a more precise definition of the terms figuring in the suggestion or by using only subjects who could be relied upon to display spontaneous amnesia for the events of hypnosis. In the latter case, special instructions would not of course be needed. It is hoped one or both of these precautions will be taken in any future work along these lines.

A second possibility is that the paramnesias evoked by our material truly represent the activity of cognitive mechanisms outside consciousness. Such an assumption is of course implied in MacCurdy's theory of *déjà vu* (5, 6, 7). MacCurdy holds that every act of recognition depends on the arousal of a co-conscious image similar in content to the present stimulus. Normally, this image is recalled into consciousness and furnishes a basis for comparison. Should it be repressed, however, through any cause, it can influence consciousness solely through the medium of affect. Thus arises the so-called feeling of familiarity. On this view, then, restricted paramnesias are due to the repression of material necessary for full recognition of any stimulus. Certainly our results could be predicted from MacCurdy's theory.

The resistance to recognition, artificially induced in our experiments, is of course closely akin to the process of repression which MacCurdy and other authors have in mind in explaining paramnesia. On the other hand, we do not ourselves believe that a pre-existing repression is necessary always for the appearance of paramnesia in daily life. In some cases, simple obliviscence seems a sufficient explanation. In other cases, the appearance of an object in an unwonted context, with difficulty in formulating the habitual context, provides a sufficient condition. What is essential is not repression as such, but any condition impeding access to the requisite memory material.

Lastly, do our experimental paramnesias throw any light on the origin of *déjà vu*? Those, who believe with MacCurdy that *déjà vu* represents a method of rationalizing paramnesia, might expect to find a certain number of our experimental paramnesias setting up further rationalizations along these lines. But in no case did we find a paramnesia rationalized in the sense of *déjà vu*. This evidence then, for what it is worth, fails to provide support for the psychoanalytic view. It may be that further experiments will enable us to duplicate also experiences of *déjà vu* under experimental conditions. But the existing results cannot be said to throw light on paramnesia in its more generalized forms.

VIII. SUMMARY

1. Illustrations are given of restricted paramnesias set up in response to material for which post-hypnotic amnesia had been induced. These responses are strikingly similar to paramnesia in daily life.

2. Three moments can usually be distinguished in the paramnesic response: (*a*) An initial judgement of reference to the past; (*b*) a phase of perplexity marking an effort of recall; (*c*) an act of rationalization often wholly constructive in character.

3. Rationalization took various forms. One of the most common was a projection of the paramnesia on to material previously seen in the waking state. A special form of this we have called inter-serial displacement. Fabulation was prominent in the responses of one subject.

4. The psychology of experimental paramnesia is briefly discussed. It is pointed out that our results might depend, in part at least, on the character of the prohibition given in hypnosis.

5. Our results provide some evidence in support of MacCurdy's theory of paramnesia. But they throw little light on the origin of *déjà vu*.

We wish to thank Prof. F. C. Bartlett and Dr J. T. MacCurdy for their interest in these experiments, Drs J. M. Blackburn and K. J. W. Craik for valuable assistance in their conduct, and Dr A. J. Lewis and Mr R. C. Oldfield for some useful criticism and suggestions.

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THE EFFECT OF ALCOHOL ON THE CENTRAL NERVOUS SYSTEM[1]

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I. *Introduction* (p. 52)
II. *Description of experiments* (pp. 53–54)
III. *Results* (pp. 54–60)
IV. *Summary and conclusions* (pp. 60–61)

I. INTRODUCTION

It is generally recognized that the effect of a given dose of alcohol on the central nervous system may vary widely from one individual to another, and also in the same individual on different occasions. Some of the factors responsible for this variation are known empirically: e.g. the dose is less effective if taken after a meal than on an empty stomach; it is more effective if taken in concentrated than in weak solution. In such cases as these, the greater effect on the nervous system is correlated with more rapid absorption of the alcohol, and it is generally assumed that the greater effect produced by such rapid absorption is due to a resulting higher concentration of alcohol in the blood than that which follows slow absorption.

In experimental work, these variable factors are eliminated as far as possible; a standard dose of alcohol is given on an empty stomach and a standard concentration is used. This treatment is applied to as large a number of subjects as possible, the psychological test is applied at a standard time after the alcohol has been taken, and the average performance of the group assessed. A more detailed study of the relationship between blood alcohol concentration and the reaction of the central nervous system on a small number of subjects has indicated that such average results can have little meaning, and that the variable factors in each individual must be taken into account if his reaction to a given dose of alcohol is to be truly recorded.

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II. DESCRIPTION OF EXPERIMENTS

Details of the methods employed for determination of alcohol concentration in blood and urine will be given elsewhere. Blood obtained from a finger prick sufficed for the alcohol analysis, and the shape of the blood-alcohol concentration curve was often amplified from analyses of the urine-alcohol concentration. The urine-alcohol concentration curve runs parallel with and about 30 % above the blood-alcohol concentration curve except during very rapid changes in alcohol concentration.

Tests of central nervous system activity. With one subject (E), on whom the majority of the experiments were performed, three tests were used; typewriting, dotting machine and 'distraction machine'. On three other subjects, only the two last tests were used.

Typewriting. A somewhat difficult passage, involving several inverted commas, etc., was chosen. The passage was always at hand for reference even when it was known by heart. Learning was slower than in the other tests; a time of 3 min. was only reduced to 2 min. 20 sec. after 2 months of almost daily practice. Thereafter, it provided a steadier base line than the other tests, varying only between 2 min. 20 sec. and 2 min. 17 sec. from day to day. In assessing the performance under the influence of alcohol, 1 error was counted as 3 sec.

Dotting machine. Learning was rapid in all four subjects. Two of the four (A and E) then showed a relatively steady performance from day to day. With the instrument moving its fastest speed, the number of correct hits recorded by A was 321 ± 2½ in his last eight consecutive tests, and by E 295 ± 3 in her last six tests. Subject C learnt slowly and remained rather erratic, 272 ± 8 in his last seven tests. Subject H showed no learning curve but remained erratic; his first six tests gave results of 253–276 (average 262 ± 4), but on the next experimental day he scored 283, and on recovering from the effects of alcohol, showed a score of 311.

'*Distraction machine.*' The general principle of this machine was as follows. A spot of light was made to oscillate, in predetermined, random fashion, about a central position on a scale. The position of this spot could also be adjusted by the subject, by the rotation of a knobbed wheel. He was instructed to endeavour to compensate the random oscillations as completely as possible, so as to maintain the spot of light at its central position. The time integral of the magnitude of the deviation from the central position for the whole period of the test (1½ min.) was obtained automatically, and recorded on smoked paper. This record was a measure of the success with which the subject was able to follow, and correct, the random oscillations. Small imperfections in the apparatus resulted in the total uncorrected deviation of the spot varying slightly from time to time. Immediately after each test, therefore, the time integral of the uncorrected deviations was recorded, and the total deviation observed during the subject's test was expressed as a fraction of this uncorrected deviation. The better the performance, therefore, the lower the score; i.e. perfect correction = zero, no correction = 1.

At irregular intervals during the course of the test (and varying in relation to each other with each test) red, green and yellow lights were displayed and a buzzing noise was made. While still manipulating the knob controlling the moving spot of light with his right hand, the subject had to respond to the buzzer by pressing a button with his left hand, and to the red light by pressing a lever with his foot, ignoring the green and yellow lights. Signals recording on the drum indicated whether the correct response had been made.

No attempt was made to compare accurately the performance with and without distractions, but in the early stage of learning, distractions appeared to reduce the performance by about 50 % (three tests, subject E). Thereafter the machine was used only in conjunction with the distractions, but it was noted that additional distractions, e.g. onlookers, a desire to cough, an uncomfortable position in the chair, etc., invariably worsened the performance.

Learning was a considerably longer process than in the case of the dotting machine. The differences noted in the learning and performance of the different subjects with the dotting machine were reflected to some extent on their performances on this instrument. Subject A learnt quickly and remained steady. His score dropped from 0·39 to 0·335 on the first day (four tests) and remained steady between 0·31 and 0·33 during the successive eight test days. Subject H was even more erratic than with the dotting machine; after starting at 0·37, he dropped to 0·315 on the 4th test day, but rose again to 0·36 on the 5th–7th days. On the 8th and 9th he varied between 0·29 and 0·32. It was learnt afterwards that he had been emotionally upset during the 5th–7th day period. Subject C learnt slowly, but ended by attaining a higher performance than Subject A, in contrast to their relative performances on the dotting machine. He settled down to a value of 0·27–0·285. No record was kept of the learning of Subject E, for she had practice during the building of the machine; a score of 0·27–0·28 was regularly obtained when accurate scoring was begun.

If small effects of alcohol are to be detected, therefore, it would seem essential to give time to the preliminary learning of the tests. If only gross changes are being investigated, this could possibly be ignored.

III. RESULTS

The degree of disturbance of the central nervous system produced by a given dose of alcohol depends on three main factors: (1) the weight of tissue in which the alcohol is distributed, (2) the length of time between administration of the alcohol and application of the test, and (3) the susceptibility of the nervous tissue to alcohol. The wide, and only partially recognized, variations in these three in different individuals nullify any attempt to standardize them.

(1) Apart from the large differences in actual weight of different people, alcohol is more widely distributed in some than in others, since it is present in only small quantity in fatty tissue. Thus, of two individuals of the same weight, but different shape, given the same dose of alcohol, the short fat one will show a higher concentration of blood alcohol than the lean muscular one, since the alcohol is distributed in a

smaller weight of tissue. A 40–50 % difference due to this cause may be met in extreme cases.

(2) The inadequacy of using a standard time interval between dose and test is due to the fact that the central nervous system is affected not only by the absolute concentration of alcohol in the blood but by the rate and direction of its change. The observations on which this statement is based are too scattered to permit the formulation of any law relating such rate of change to degree of disturbance of the nervous system, but illustrations of the different effects observed serve to show the order of magnitude of the relationship.

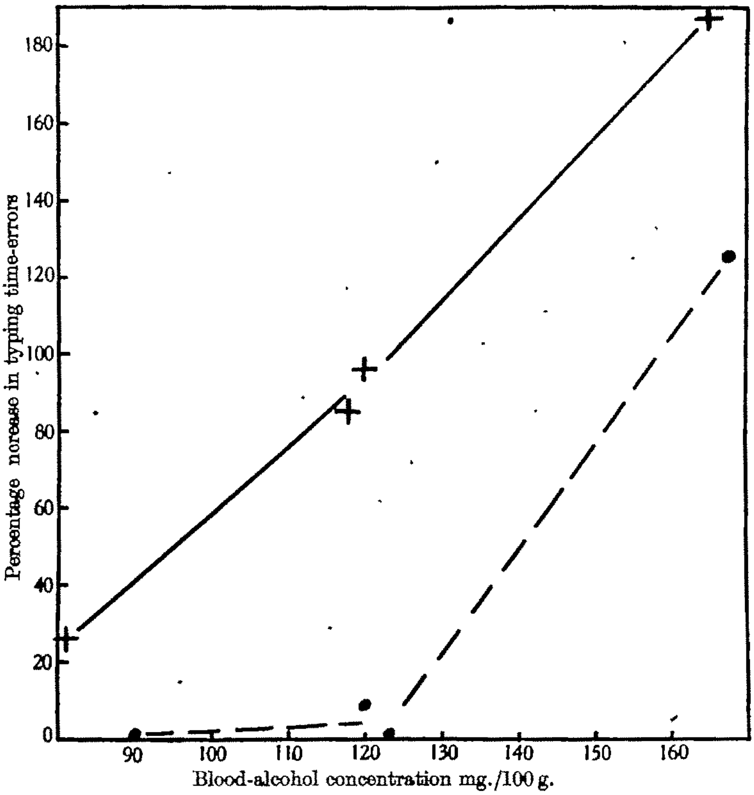

Fig. 1. The effect of blood-alcohol concentration on typing time-errors.
+———+ concentration increasing. ●– – – –● concentration decreasing.

The dependence of central nervous system disturbance on the absolute value of blood-alcohol concentration is shown, more or less quantitatively, in Fig. 1, and with it the effect of increasing and decreasing concentration. The experiments were all performed on the same subject. With increasing concentration the typing time-errors were practically doubled at a concentration of 120 mg./100 g. blood, but at this same concentration recovery was almost complete when the concentration was decreasing.

Fig. 2 shows the effect of *rate of increase* in blood-alcohol concentration on the performance with the Distraction Machine in one subject. At the same absolute concentration (100 mg./100 g. blood) an increase in errors of only 13 % was observed on a slow-rising blood-alcohol curve compared with a 70 % increase on a very fast-rising one.

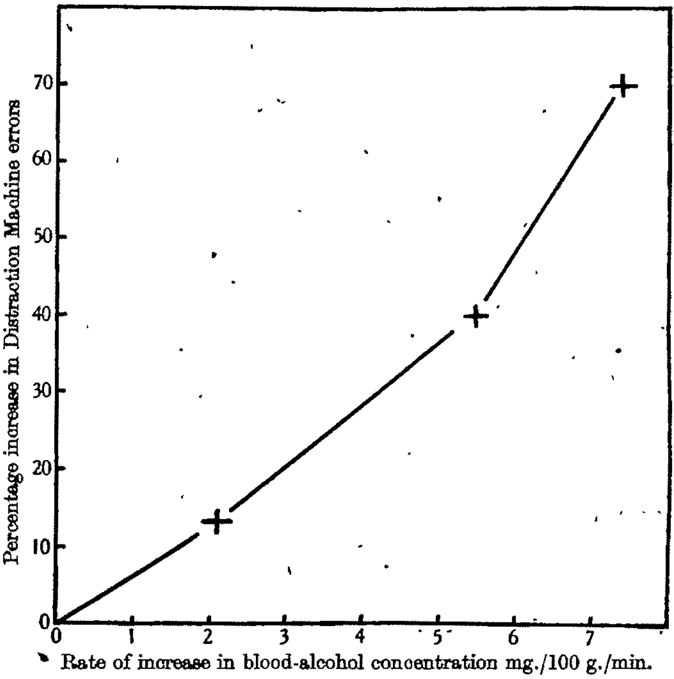

Fig. 2. The effect of *rate* of increase in blood-alcohol concentration on Distraction Machine errors. All three tests were made at an absolute concentration of 100 mg./100 g.

This effect is illustrated from another point of view in Fig. 3, which shows the relationship between absolute blood-alcohol concentration and its rate of increase for a given degree of central nervous system disturbance. This was not quite constant, but the least disturbance (65 % increase in typing time-errors) occurred at a concentration of 180 mg./100 g. blood on a slow-rising blood-alcohol curve and the greatest (90 % increase) at only 115 mg./100 g. blood on a fast-rising curve.

Information regarding the effect of rate of decrease in blood-alcohol concentration is less decisive, for this rate has not varied within wide limits in different experiments. The phenomenon illustrated in Fig. 4, however, has been met several times and suggests that a high rate of decrease is accompanied by more rapid recovery of the nervous system than is a low rate. If, for any reason, there is a delay in the rate of disappearance of alcohol from the blood stream, the recovery of the central nervous system is interrupted as indicated by the test performance. The effect of increasing compared with decreasing blood-alcohol concentration can also be seen in this figure. An increase in errors on the Distraction Machine of 50 % at about 110 mg./100 g. on the rising blood-alcohol curve is reduced to 20 % at the same concentration on the falling curve.

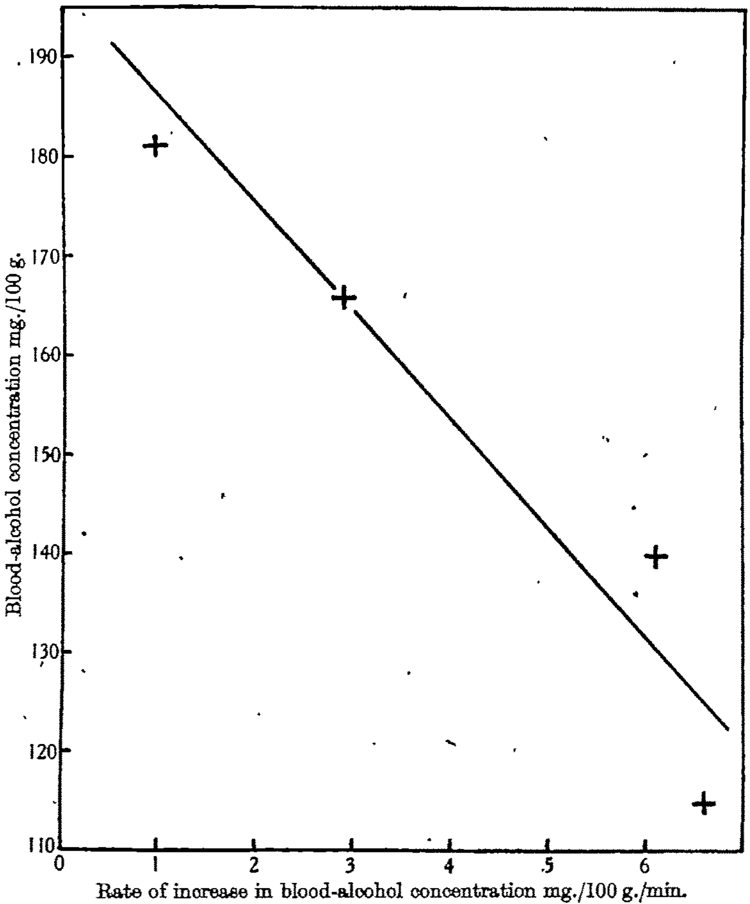

Fig. 3. The relationship between rate of increase of blood-alcohol concentration and the concentration at which a given deterioration in performance (65–90 % increase in typing time-errors) occurs.

One obviously important factor in controlling the regularity of change in blood-alcohol concentration is the behaviour of the stomach. The retardation of recovery shown in Fig. 4 was probably due to the extrusion of a small quantity of alcohol left behind in the stomach, followed by its rapid absorption in the small intestine.

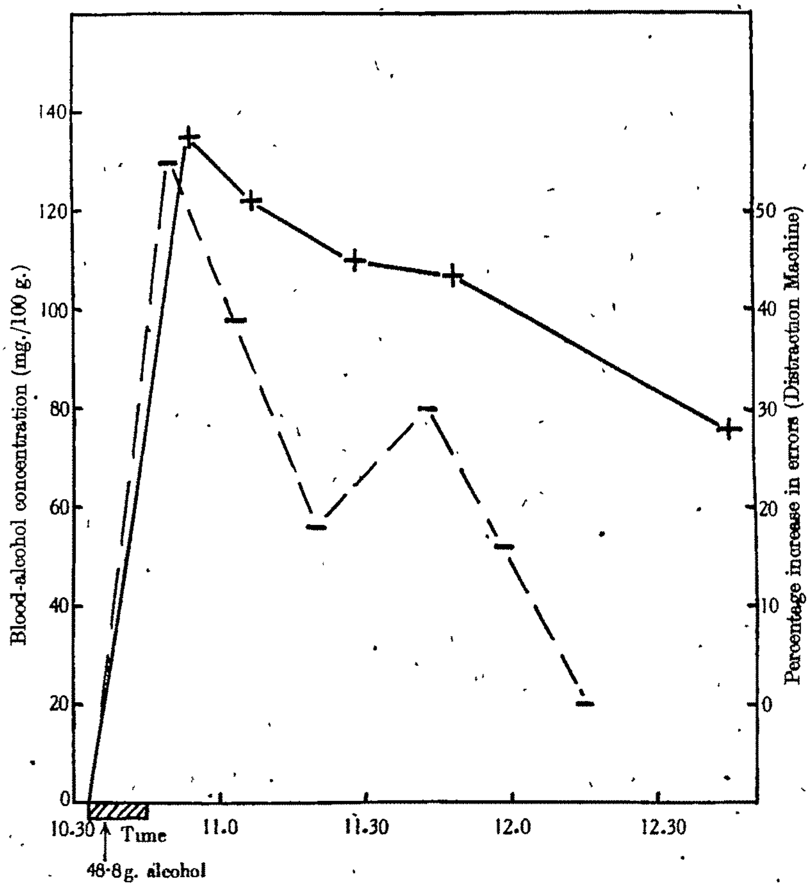

Fig. 4. Comparison of performance on Distraction Machine with blood-alcohol concentration, showing the interruption in recovery accompanying a diminution in the rate of decrease in blood-alcohol concentration. +———+ blood-alcohol concentration. — — — — percentage increase in Distraction Machine errors.

The effect shown in Fig. 5 is undoubtedly due to the same cause and has also been met several times. It can be produced in still more pronounced fashion by giving the drink ice-cold. Recovery of the central nervous system as indicated by typing time-errors was almost complete as the blood-alcohol concentration fell to 116 mg./100 g., although a

35 % increase in errors had been observed at a concentration of only 80 mg./100 g. on the rising curve. As the blood-alcohol concentration rose again, an increase of over 50 % in time-errors occurred at 135 mg./100 g.; at 120 mg./100 g. recovery was again complete.

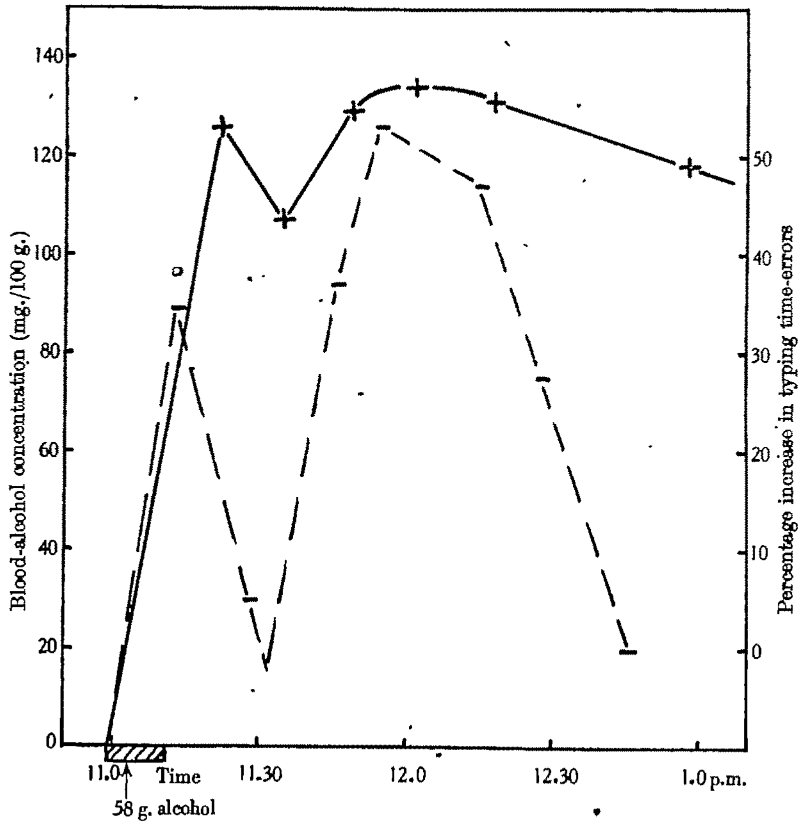

Fig. 5. The relationship of performance in typing test to changing blood-alcohol concentration, showing the greater effect on the central nervous system of a given blood-alcohol concentration when this is increasing than when it is decreasing. +———+ blood-alcohol concentration. – – – – – percentage increase in typing time-errors

These last two figures illustrate well the main point under discussion. Had the test been performed in each case 30 min. after the alcohol was administered, the larger dose shown in Fig. 5 would have, apparently, produced no deleterious effect on the nervous system, and the smaller dose shown in Fig. 4 a quite marked effect (30–40 % increase in errors). In neither case would the test have indicated the maximum effect produced by that dose on the nervous system. When tests are made for the purpose of assessing the effects of alcohol on a given individual, therefore, particularly if the result is of practical importance, it would seem advisable to apply the test at least every 10 min. for the first hour following the dose, or until recovery is complete. The subject will in many cases know his own limitations even though he has no knowledge of the blood-alcohol concentration curve: e.g. the habitually rapid absorber may normally take his drink a good half-hour before starting on his work.

The subject from which the records shown in these figures were taken was naturally a fast absorber, the fastest of the four subjects used. In general, each subject appeared to have a natural rate of absorption, and the comparison of various rates was more easily made on the naturally fast one, since absorption can be more readily slowed than hastened. It seems clear that one important factor in the varying tolerance of different individuals to the same dose of alcohol is their normal differing rate of absorption.

(3) To what extent the central nervous system of different individuals may vary in resistance to a given concentration at a given rate of increase is not known. It is, however, a matter of common belief that such is the case, but accurate comparison has not been possible in the series of experiments reported here. Similarly, no accurate evidence is available as to whether this difference in individual resistance of the central nervous system to alcohol, if it exists, is inborn or acquired by frequent usage of alcohol.

IV. SUMMARY AND CONCLUSIONS

It has been shown that the degree of disturbance of central nervous system function, as indicated by performance on typewriter, dotting machine and Distraction Machine, is affected not only by the absolute concentration of alcohol but also by the rate and direction of its change. This statement is based on the following sets of observations:

(1) At any given concentration of blood alcohol, the nervous disturbance is greater if this concentration is increasing than if it is decreasing.

(2) The degree of nervous disturbance at any given blood-alcohol concentration increases with the rate at which this concentration is increasing.

(3) The rapid recovery of the nervous system accompanying a decrease in blood-alcohol concentration is impeded if the rate of decrease is reduced.

The application of a single test at a given time after a dose of alcohol is, therefore, not likely to yield accurate information as to the magnitude of its effect in that subject. It is suggested that a test repeated every 10 min. during the hour following the dose of alcohol, or until recovery is complete, is advisable in all cases where the result is of practical importance.

The Distraction Machine was made for me by L. E. Bayliss, to whom I am also indebted for his constant help and interest in the work.

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THE NATURE OF DARK ADAPTATION

By K. J. W. CRAIK and M. D. VERNON

From the Psychological Laboratory, Cambridge

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I. Introduction

Visual adaptation is the reduction in absolute sensitivity of the eye which occurs during and remains immediately after stimulation by a bright light, and the increase in sensitivity which occurs in the dark. The dark adaptation curve has been shown by Kohlrausch (1922) to consist of two portions, the first representing cone and the second rod adaptation; the first is characterized by perception of colour at the threshold (cf. Hecht, 1921; Hecht, Haig & Chase, 1937) and fairly good acuity; the second by an increase in rate of adaptation as the rods begin to operate, but very low acuity and absence of colour-vision. It has been shown (Craik, 1938) that the eye, when adapted to a certain illumination, responds to changes of illumination over a range of illuminations a thousand times greater or less than this adapting intensity, and that adaptation shifts this instantaneous range up or down the scale of physical illuminations. Thus adaptation is a form of automatic 'range-setting' on the part of the eye which enables it to respond most sensitively to changes in the illumination to which it has become adapted.

It is important to decide where this process of adaptation occurs. There is much physiological evidence that such an effect is shown by the isolated eye and optic nerve (e.g. Hartline & Graham, 1932; Hartline, 1938) and that the concentration of visual purple in the retina is reduced by intense illumination (e.g. Kühne, 1879) and probably accounts for the reduction in sensitivity. However, the electrophysiological or photochemical measurements of adaptation cannot be correlated exactly with its sensory effects, and some minor anomalies arise (Granit, Munsterhjelm & Zewi, 1939). In addition, Brown & Page (1939) have suggested that the initial or cone portion of the dark adaptation curve has a cortical origin and many psychologists have shared the same view on account of the complicated oscillations and inhibitory phenomena which negative after-images or bright adapted areas of the retina sometimes show. Thus some more direct proof of the retinal or cortical origin of the adaptive process seems desirable.

Helmholtz (1924, p. 7) was aware that moderate pressure on the eyeball causes temporary blindness, and quotes Donders as having shown ophthalmoscopically that pulsation and emptying of the retinal blood vessels occurs under these conditions. This has been confirmed by Duke-Elder (1926). This blindness affords a convenient method of blocking the transmission of nerve impulses between the eye and brain. It probably acts by producing retinal anoxia and failure of excitation or transmission in the retinal nerve endings. The bright and dark adaptation of the human eye thus isolated from the cortical visual centres during a period of darkness, during exposure to bright light, or during the immediately succeeding period have therefore been investigated by studying the sensory response immediately after it has returned on releasing the pressure.

II. DESCRIPTION OF APPARATUS

(1) *Requirements of apparatus*

Any dark adaptation apparatus must consist of two parts, the system for bright adapting the subject in order to annul the effects of previous light or darkness and the system for measuring the absolute threshold. A 'Maxwellian view' (see Craik, 1939*a*, p. 253) permits high bright-adapting illuminations to be produced over a fairly large field from a conveniently small light source. It also produces less heat and discomfort than a screen lighted by powerful lamps. The image cast on the pupil of the eye can also be used as an artificial pupil and permits a larger angular diameter of field than is possible with the usual type of artificial pupil-stop placed before the eye. The testing field must be rapidly substituted for the bright adapting one, preferably without movement on the part of the subject whose head should be fixed in a rest. The test field should be 10° or so in diameter and should be capable of being made smaller and of varying in brightness from 10^{-6} e.f.c. upwards to about 1 e.f.c. without marked alteration in its spectral composition. A reducing device having a logarithmic scale is an advantage. A convenient method of recording the illuminations which are just visible to the subject at each moment is also required.

(2) *Details of apparatus*

A 100 W. 200 V. projection bulb *a* (Fig. 1) is mounted in a ventilated metal box *b*. It illuminates a ground glass diffusing plate *c* through a calorex filter *d*, the area being governed by the aperture stop *e*. Condenser lenses *f* and *g* of 2·5 cm. focus, placed 30 cm. from the diffusing glass cast an image of the stop *e* 2·2 mm. in diameter on the subject's pupils at *h* and *i*. He sees a field 56° in diameter having a brightness of 30,000 e.f.c. as seen through a 2·2 mm. pupil. A blackened gauze at *j* reduces this to 12,000 e.f.c. The right-hand eye-lens is moved in a curved slide by a micrometer screw *k* to allow for variations in inter-ocular distance. The box *b* is suspended by a system of links *l* like those on a parallel ruler and remains in the up or down position by being counterweighted at *m*. Chains *n* co-ordinate the movements of the links near the point of dead-centre. Artificial pupils are placed at *o* so that they coincide in height and position with the pupil-images of the bright adapting system when the box is raised; these artificial pupils can be moved out of the way and the right-hand one is adjustable for inter-ocular distance. A chin rest *p* adjustable in three dimensions by means of a lathe slide-rest is fixed to the base *q*; wax impressions of the subject's teeth can be fitted instead. An adjustable cradle *r* slung from the base-board carries the projection system for the test field. This consists of a lantern *s* with a 3 W. 6 V. car side-light bulb, condenser, and objective in focusing mount. A neutral gelatine wedge covering 6 log units of density is held in a brass frame *t* near the objective and operated by a rack and pinion *u*. A rod on the end of the sliding frame carries a thin soft pencil lead sprung in a tube *v* which records the position of the filter continuously on a paper

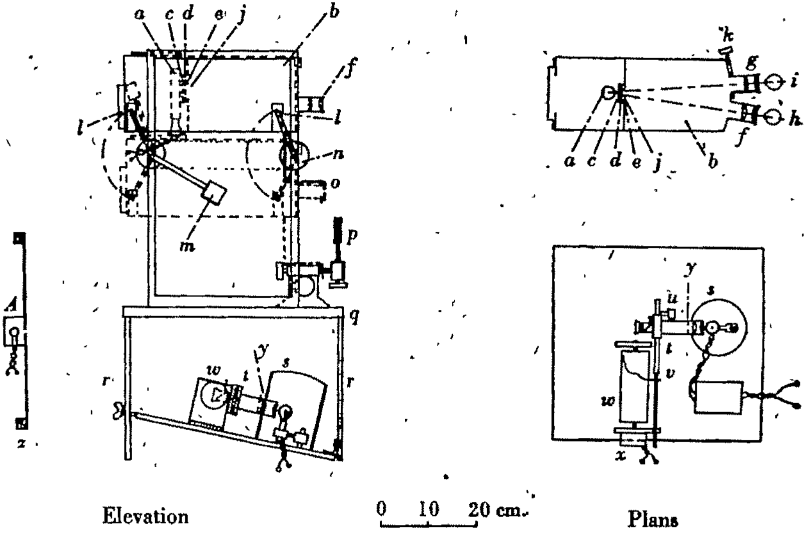

Fig. 1. Diagram of apparatus. For description see text.

clipped on the drum w driven by a synchronous electric clock movement x so that it makes one revolution in an hour. The size of the field is determined by a stop placed at y; its diameter was 10° without, and 6° with the stop. The lantern illuminates a screen z of matt white card placed 1 m. from the objective. At its centre is a fixation point consisting of a pinhole behind which is placed a 1 W. 4 V. bulb A controlled in brightness by a resistance. This lamp and that in the test-field lantern are fed by bell-transformers.

(3) *Calibration*

The maximum illumination on the screen was measured by a Ritchie wedge photometer and by a Holophane lumeter and was found to be 0·27 f.c. after correction for the reflection factor of the screen. The brightness was 0·22 e.f.c. as viewed through a 2·2 mm. pupil. The bright adapting fields were calibrated by measuring the total energy in the pupil-image by a generator type selenium oxide photocell and micro-ammeter. This reading was then duplicated by illuminating an opal glass screen of known transmitting properties by a 600 c.p. projector lamp with a calorex filter interposed, the light falling on the cell being limited by a 2·2 mm. artificial pupil. The illumination on the opal screen was calculated from the distance of the projector bulb. For checking purposes a photocell with a mount enabling it to be clipped to the lantern or bright adapting eyepieces was made; this gave a standard reading which could be verified on subsequent occasions. The test and bright adapt illuminations were subject to a maximum variation of ± 10 %, owing to mains voltage fluctuations; this is not serious in comparison with the variability of visual thresholds.

For applying pressure to the eye an instrument resembling a tonometer or spring balance was used. A light plunger having a sponge rubber plug 1 cm. sq. in its end moved against a spring in an outer tube. Two springs, one giving a range of 0–250 g. per sq. cm. and the other 0–700 g. per sq. cm. were used and the apparatus calibrated with weights. Tonometers do not indicate intra-ocular pressure accurately, owing to the relatively large influence of the stiffness of the cornea or sclerotic. A calibration in mm. Hg, accurate to ± 10 mm., was secured by applying the instrument to the eye of a freshly killed cat, into which a cannula had been inserted and connected to a mercury manometer. While each pressure was applied the manometer was adjusted so that the volume of fluid in the eye was constant, and the height of the column of mercury recorded. The range covered (from 35 to 225 mm. Hg) may be compared with the normal intra-ocular pressure (about 25 mm. Hg) and the ocular blood pressure (about 27 mm. Hg with a pulse amplitude of only 2 mm. Hg).

On the human subjects any required pressure could be applied and maintained with an accuracy of ± 10 g. by grasping the instrument with the first finger in the appropriate position on the tube and pressing until the pointer on the instrument just touched this finger. To avoid damage to the sclerotic the pressure was applied through the upper lid near the outer canthus.

III. PROCEDURE AND RESULTS

In order to take a dark adaptation curve the subject was seated in front of the apparatus and the chin rest and artificial pupils adjusted so that he could see the test field with both eyes, this field and the fixation point being both at their maximum brightness. The bright adapting apparatus was then lowered into position and the right eye-piece adjusted until both eyes when opened in turn saw a uniformly bright field. The field was then fixated for 3 min., this period being found experimentally, to produce complete adaptation to any illumination.[1] The bright adapting apparatus was then moved up and the light turned off, the clock switched on, and the subject, in accordance with instructions given beforehand, moved the knob controlling the filter wedge so as to keep the test-field just visible. He also reduced the fixation point brightness with his other hand till it was just bright enough to indicate the position of the test field. (It was found that if the fixation point, in the latter stages of adaptation, was 100 or 10,000 times as bright as necessary a 50-fold or 100-fold rise in threshold was produced by its glare effect.) A continuous record of the logarithm of the threshold illumination against time was thus obtained for the succeeding 55 min. One subject was used, with some confirmatory readings on a second. Three experiments with pressure were performed:

(1) The time of onset of blindness produced by pressure alone and the recovery time on release of pressure were determined with a stop-watch.

(2) The eye was fully dark adapted, then bright adapted for 3 min. at 12,000 e.f.c. while completely blinded by a pressure of 250 gm. per sq. cm. and a dark adaptation curve taken after releasing the pressure.[2]

(3) The eye was bright adapted in the normal way, then a pressure of 400 gm. per sq. cm. was applied for 3¾ min. (it was thought that a longer period might cause injury) and a dark adaptation curve obtained immediately afterwards.

The curves relating pressure and time of onset of blindness or 'disappearance time' (Fig. 2) resemble rectangular hyperbolas which are shifted along the time axis with variation in the illumination employed. The actual values of the times obtained are given in Table 1. For any given illumination there is a certain minimum time below which no pressure however great causes a reduction in the disappearance time and a certain minimum pressure below which no blindness is caused however long the pressure is maintained. With small fields (e.g. about 10°) eye-movements result in a marked return of vision and almost exactly double the disappearance times.

---

[1] It was found that, provided this condition was observed, daily variations in the dark adaptation curve did not exceed a range of 0·2 log unit.

[2] Difficulty was experienced in keeping the eye opposite the pupil-image of the bright adapting apparatus when it was displaced sideways and blinded by pressure. This was overcome by aligning the head so that the bright adapting field, reduced to 1 e.f.c. by a series resistance, was clearly seen with pressure applied. Blindness commenced after about 10 sec. When 30 sec. had elapsed and the eye was blind even to the highest intensities the series resistance was cut out and the 3 min. bright adaptation given. The bright adapting effect of the 10 sec. exposure to 1 e.f.c. is negligible in comparison with the effect of 12,000 e.f.c., as will be shown in Part III of the present series.

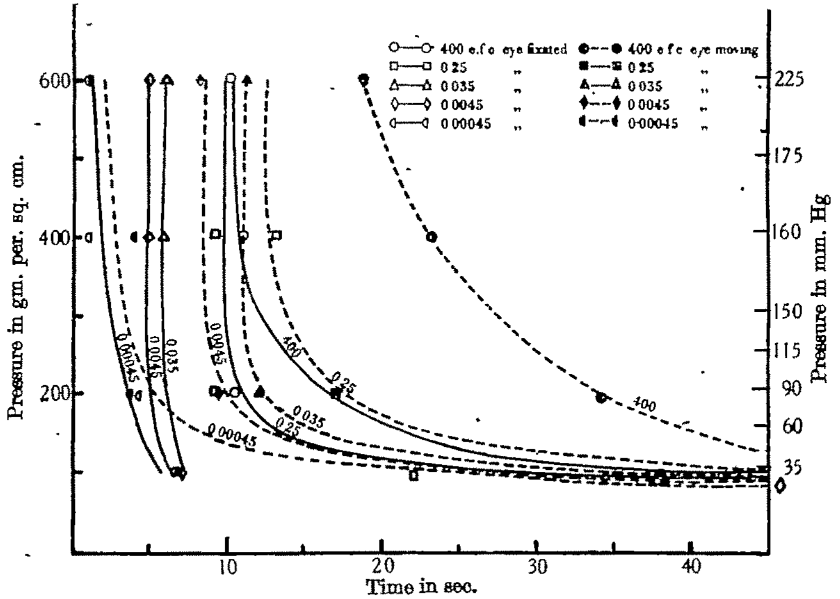

Fig. 2. Relation between pressure applied to the eye and disappearance time for different illuminations, for fixed (solid line) and moving vision (dotted line).

The recovery of vision on releasing pressure is considerable in 5 sec. and complete (as measured by return to the previous value of the dark-adapted threshold) in 3 min., as shown in Fig. 3.

Table 1

Brightness of test patch in f.c.	Extinction time for fixated eye at pressure (in mm. of Hg)				Extinction time for moving eye at pressure (in mm. of Hg)			
	225	160	80	35	225	160	80	35
400	10	11	10½	38	18½	23	34	—
0·25	10	9	9	22	(20)	13	17	62
0·035	6	6	5	7	11	11	12	48
0·0045	5	5	4	6	8	9	9	47
0·00045	1	1	4	6	1	4	4	54

When the eye had been blind during bright adaptation the dark adaptation curve obtained on release of pressure differed in no way from normal except for the slight initial delay attributable to the recovery from the pressure itself (Fig. 4).

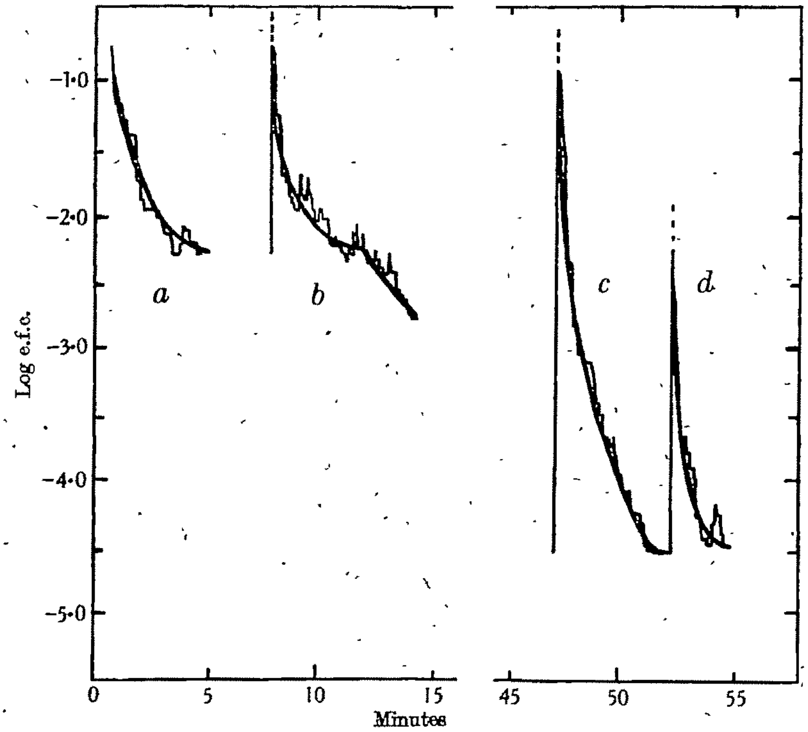

Fig. 3. Recovery of vision after pressure alone: curve *b*, recovery on flat portion of cone adaptation curve (*a*) after 3 min. pressure. Curves *c* and *d*, recovery on flat portion of rod curve after 1½ and 1 min. pressure respectively. In these and subsequent figures the jagged lines are the actual excursions of the recording pencil.

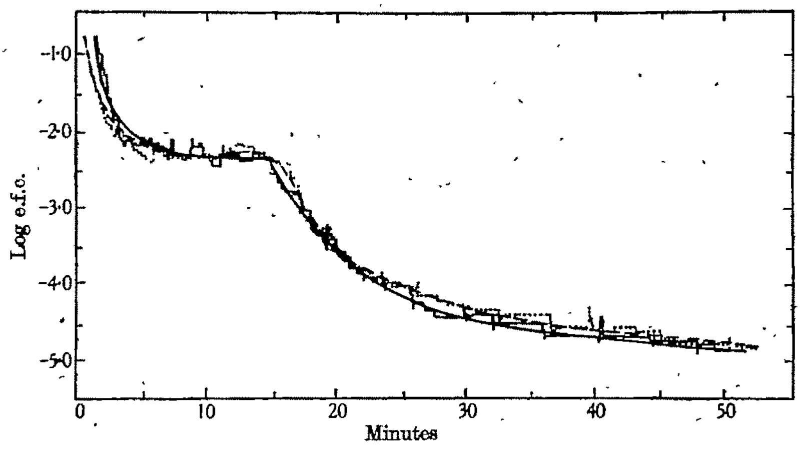

Fig. 4. Solid line—dark adaptation curve when eye had been blind during bright adaptation period; broken line—normal dark adaptation curve for comparison.

When the eye was exposed to pressure for 3¾ min. immediately after normal bright adaptation the rod and cone portions of the curve were not delayed (Fig. 5). There is an apparent lag in cone adaptation, but this is explicable as a recovery of the neural system concerned from the pressure itself, with the time course of which it agrees (cf. Fig. 3).

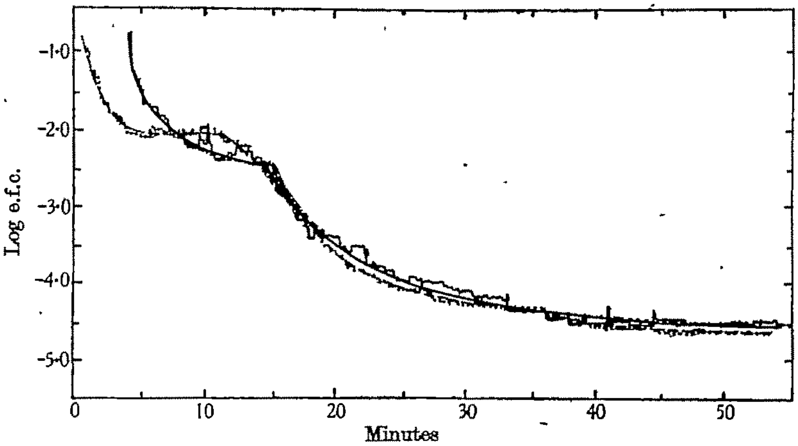

Fig. 5. Solid line—dark adaptation curve when eye had been pressed for first 3¾ min. after normal bright adaptation; broken line—normal dark adaptation curve for comparison.

IV. Discussion

The curves showing the relation between pressure and disappearance time for various illuminations indicate that pressure causes a rise in threshold which commences after 5–15 sec. and increases for 30 or 40 sec. Waite, Derby & Kirk (1925) found a rise in the absolute threshold in established glaucoma, which may be partly due to the same action of the raised intra-ocular pressure. Ophthalmoscopic observations of the pulsation and emptying of the retinal arteries caused by pressure were also made in the present investigation. The simplest explanation of the blindness is that the stoppage of circulation produces retinal anoxia and nerve-block, probably in the ganglionic layers. The effect cannot be photochemical since pressure alone does not reduce the photochemical sensitivity of the retina; the recovery of dark adapted sensitivity in 3 min. after release of pressure, as compared with the 30 min. or so required for recovery following bleaching of the visual purple by full light adaptation, is proof of this. The minimum pressure which produces any blindness is about 80 mm. Hg, which is the same as that found by Duke-Elder (1926) to produce slight occlusion and intermittent flow in the retinal arteries. He found that pressures above 115 mm. Hg produced complete obliteration of the arteries. This corresponds quite well with the 'knee' in the present curves, above which increase in pressure up to the maximum of 225 mm. Hg produces little further reduction in the disappearance time.

The temporary return of vision with eye-movements is explicable in terms of local photochemical adaptation which has been shown to occur in the retina in the region of a bright image by Kühne in his 'optograms'. Such local bleaching would reduce the sensitivity in that part, while eye-movements expose more sensitive areas the response from which can still pass through the region of relative nerve-block.

It appears, therefore, that pressure on the eye provides a convenient method of temporarily blocking the neural connexion between eye and brain and of stopping the retinal circulation without causing disturbances in the concentration of photochemical substances in the retina at the time when pressure was applied. Release of pressure permits recovery in a period very short compared with that of dark adaptation, of the positive after-images resulting from intense stimulation, and of other effects which one may wish to study. Thus pressure furnishes a convenient 'tap' between eye and brain which can be turned on or off at will.

The normality of the dark adaptation curve obtained with an eye which has been blind during the bright adaptation period shows that bright adaptation both of rods and of cones is a retinal reduction of sensitivity and not a cortical one, since the primary stimulation never reached the higher centres.

The absence of delay in dark adaptation caused by a period of pressure shows that circulation—e.g. oxygenation—is not necessary for the regeneration of visual purple and cone substances in the human eye and suggests, even, that the blood may play no part in this regeneration.

V. SUMMARY

Apparatus for detailed investigation of dark adaptation is described. Temporary blindness produced by mechanical pressure on the eye (occluding the arteries and presumably causing retinal anoxia and nerve-block) affords a convenient method for separating processes occurring in the eye and brain. If a dark adapted eye is bright adapted while thus blinded the subsequent dark adaptation curve is the same as if the eye had been bright adapted while vision was normal. This confirms that bright adaptation both of cones and of rods is a retinal process. Dark adaptation is not delayed by pressure applied immediately after normal

bright adaptation, indicating that circulation is not essential for photochemical regeneration. The effect of maintained pressure is to cause a steady rise in threshold which increases for 30–40 sec. The least pressure producing any loss of vision is about 80 mm. Hg, while pressures from 300 to 440 mm. Hg produce complete blindness in a fixed period, indicating complete stoppage of circulation.

PART II. THE PART PLAYED BY THE POSITIVE AFTER-IMAGE

I. *Introduction* (pp. 71–72)
II. *Apparatus* (pp. 72–73)
III. *Procedure* (p. 73)
IV. *Results* (p. 73)
V. *Discussion* (pp. 74–75)
VI. *Summary* (p. 75)

I. INTRODUCTION

WHEN the eye has been exposed to a bright light the absolute threshold is found to be raised above its dark adapted value for a considerable period afterwards. The later or rod portion of the dark adaptation curve certainly results from regeneration of visual purple and the consequent increase in the sensitivity of the eye. The earlier portion, however, coincides initially with the period during which a bright positive after-image is seen, so that the rise in threshold might be partly due to the masking effect of this after-image, just as an illuminated background raises the value of the threshold above its absolute value to the value of the differential threshold at that background illumination. The absolute threshold, in other words, may be in some respects a differential threshold for visibility of the test field against the background provided by the positive after-image. It is important for theories of dark adaptation that this masking effect should be separated from 'true' adaptation or reduction in sensitivity of the receptors, since the theory of masking must be in terms of differential thresholds against the background or after-discharge provided by the after-image, while the true rise in sensitivity must be regarded as indicating a reduction in photochemical or neural sensitivity of the receptors concerned. Some method of separating these two effects is therefore desirable.

It is conceivable that light adaptation by lights of different intensities and durations alters the ratio of masking effect to threshold rise; but the effect is not sufficiently marked to differentiate the two phenomena clearly.

A more promising method is to measure the equivalent brightness of the positive after-image by projecting it on to a dark area with a surround whose illumination can be varied so that a match is obtained at each instant. If we then assume that the masking effect of the positive image is the same as that of a physical field having the same brightness we can estimate the rise in absolute threshold due to this cause from data on the threshold measured against a physically bright background (e.g. the brightness-discrimination data of Steinhardt (1936) for test areas of similar size) and can compare it with the actual value of the absolute threshold at each moment.

Helmholtz (1924, p. 236) pointed out that a positive after-image will be invisible if projected on a uniformly bright background whose physical illumination is such that

$$H = aH + I$$

where H is the subjective brightness of this uniform field around the area of the positive after-image, I the brightness of the positive after-image, and aH the reduced subjective brightness of the field in the area of the positive after-image. This reduction in subjective brightness is of course due to the reduction in visual sensitivity which follows previous light adaptation, and which we are endeavouring to separate from the masking effect. If we also obtain the background brightness, at each moment, for the brightness of the positive after-image projected on a dark area, we have

$$H_2 = I$$

and might obtain the reduction factor a by subtraction of the two equations. This, however, only gives the reduction in visual sensitivity at the brightness H or H_2, where it is already greatly below its maximum; and other errors render the method too inaccurate. The present results, therefore, were obtained by the first method.

II. APPARATUS

Monocular vision was used. The bright adaptation of a large area of the whole retina, or of a $3\frac{1}{2}°$ field, was obtained by a system giving a Maxwellian field of 3000 e.f.c. and an artificial pupil 2 mm. in diameter. After 3 min. bright adaptation the subject, wearing an artificial pupil of the same size inserted in a spectacle frame, looked at an opal glass surface having a $3\frac{1}{2}°$ black disc at its centre and illuminated from behind by a 1 W. bulb. The maximum brightness of this surface, which subtended 15°, was 70 e.f.c.; it could be reduced by a variable resistance to 0·001 e.f.c. On the shaft of the resistance was mounted a pinion which moved a rack with a pencil recording on a polygraph (Craik, 1939). A marker magnet operating another pencil recorded time at 5 sec. intervals. The variation of brightness with resistance was measured with a Holophane lumeter; the scale was found to be nearly logarithmic. The colour change of the light was not a serious disadvantage since the after-image itself changes from impure green to impure brown at intervals and the brightness match can only be approximate under the best conditions.

III. PROCEDURE

The subject fixated the 3½° bright adapting field for 3 min., then rapidly projected it on to the black disc and modified the brightness of the surround so as to match the image as nearly as possible until the image had completely vanished. This was found to be possible within an accuracy of ±50 %, or better after some practice—i.e. variations larger than these were recognized and corrected by the subject. The course of the positive after-image could usually be followed for about 3 min. The absolute threshold with a test field of similar size was then measured, after 3 min. bright adaptation to the bright adapting field with a field stop removed, so that it had a diameter of 66°. Three subjects were employed.

IV. RESULTS

A typical curve showing the time course of the positive after-image is shown in Fig. 6. The reduction in absolute sensitivity and the portions attributable to true reduction of sensitivity and to the masking effect of the positive after-image are given in the Table 2. They were calculated by the method described above, but can only be regarded as approximate.

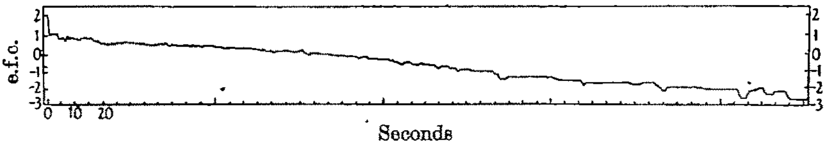

Fig. 6. Time course of positive after-image.

Table 2

(Mean results for three subjects)

Time	Calculated true reduction		Masking effect		Total reduction i.e. 1/threshold rise above value after 5 min.
10″	0·0085	×	0·36	=	0·003
30″	0·22	×	0·12	=	0·026
1′	0·21	×	0·3	=	0·064
1′ 30″	0·35	×	0·6	=	0·21
2′	—		—		0·4

V. DISCUSSION

It appears that the rise in threshold found within the first 10 sec. after bright adaptation is mainly due to true reduction of sensitivity. By the end of 30 sec. the positive after-image has increased in brightness, and for the next minute it and true reduction of sensitivity appear to play approximately equal parts. At the end of 2 min. the positive after-image has decreased considerably and further measurement is difficult. It appears that the remaining elevation of the cone threshold above its final value is mainly due to a true reduction in retinal sensitivity.

While these results are necessarily inexact they indicate that masking by the positive after-image plays a considerable part in determining the form of the dark adaptation curve. They give ground for caution in proposing precise photochemical theories, such as those of Hecht, which claim to decide the order of the reaction involved in cone dark adaptation, in disregard of the alteration of the curve by the positive after-image effect.

It is impossible to estimate the part played by the positive after-image in the rapid recovery of rod dark adaptation after exposure to low illuminations such as 1 e.f.c., owing to the feebleness of the image under these conditions. It would appear, however, that it is a small factor and that the whole of the threshold rise certainly cannot be attributed to it.

The time course of the positive after-image itself as shown in Fig. 1 raises some interesting problems. At first sight, the positive after-image might seem to be a simple retinal after-discharge. This view is untenable, however. The subjective brightness, and presumably therefore the maximal photochemical and neural response (cf. Hartline & Graham, 1932), is highest during the first 5 sec. of exposure of a dark adapted eye to an intense light. Yet the positive after-image has a much longer duration after the eye has become adapted to the high illumination (e.g. in about 1 min.) and its subjective brightness has fallen to a much lower value. Closer examination shows that there is an intense, momentary after-image following high illumination and that it is much less intense when bright adaptation is complete. This may be called the true after-discharge; it lasts less than 1 sec. After short light exposures (e.g. 5 sec. at 3000 e.f.c.) this after-discharge is followed by a short period of complete darkness, after which the main positive after-image appears, and vanishes in another 20 sec. or so. After exposures lasting 1 min. or more this after-discharge is less marked, there is no dark interval, and the main positive after-image is intense and prolonged, enduring for nearly

5 min. This after-image is visible even though the eye has been bright adapted while rendered blind by pressure, as described in Part I, so that it must have a retinal and not a cerebral origin. The most reasonable explanation seems to be that it is connected in some way with the regeneration of the photoproduct, whose concentration is clearly maximal after prolonged exposure to a high illumination. There is no electrophysiological evidence of retinal or nerve after-potentials of such long duration, though the primary response to physical illumination having an intensity equal to the equivalent brightness of the positive after-image in man would be measurable. Since, however, conger eels (Adrian & Matthews, 1927), frogs and snakes (Hartline, 1938) are the only vertebrates in which optic nerve potentials have so far been recorded, it is possible that the evidence is not comparable with subjective results obtained on man.

VI. SUMMARY

The part played by the positive after-image in masking dim test fields and so contributing to the rise of threshold after bright adaptation has been investigated by a matching method which enabled the equivalent brightness of the positive after-image to be measured and its masking effect roughly assessed. It is found to contribute about half the cone threshold rise during the first 2 min. following bright adaptation at 3000 e.f.c. This positive after-image does not seem to be a simple after-discharge but is perhaps connected with the regeneration of a photoproduct in the cones.

PART III. EVIDENCE FROM VARIATIONS IN THE INTENSITY OF THE BRIGHT ADAPTATION AND OF THE SIZE OF THE TEST FIELDS

I. *Introduction* (pp. 75–76)
II. *Apparatus* (p. 76)
III. *Results* (pp. 76–78)
IV. *Discussion* (pp. 78–80)
  (1) *Effect of variation in bright adapting intensity* (pp. 78–79)
  (2) *Dark adaptation with* 10 *in. field* (pp. 79–80)
V. *Summary* (pp. 80–81)
References (p. 81)

I. INTRODUCTION

THE mechanism of dark adaptation should be elucidated by varying the conditions under which it is measured. For instance, if dark adaptation consists in regeneration of a photosensitive substance at a rate determined by the concentration of photoproduct at any instant, the curve showing recovery after various degrees of light adaptation should be constant in shape; the point of commencement will merely be shifted up or down the curve. Hecht, Haig & Chase (1937) carried out an investigation over a limited range of bright adapting intensities.

Any variation in threshold with size of test field should also indicate how far the photochemical mechanism is the essential one, and how far summation between different receptors in the retina is involved.

II. APPARATUS

The apparatus used for investigating the effect of variation in the bright adapting illumination was the same as that described in Part I, except that the brightness of 100,000 e.f.c. was obtained by substituting a 250 W. projector bulb for the customary 100 W. For the investigation of variations in test field size a higher field brightness is required than could be obtained with the lantern of this apparatus. An instrument was therefore employed in which the test and bright adapt fields are both obtained by Maxwellian view from one light source. The maximum brightness of the test field was 0·01 e.f.c. It subtended 10 min. of arc at the subject's eye. A 2 mm. artificial pupil was used throughout. Three subjects were used in each investigation.

III. RESULTS

A family of dark adaptation curves from one subject at different bright adapting illuminations is shown in Fig. 7 and curves at 12,000 and 100,000 e.f.c. from another subject in Fig. 8.

Fig. 9 shows dark adaptation curves taken after 3 min. bright adaptation to 12,000 e.f.c. (preceded by dark adaptation) and after

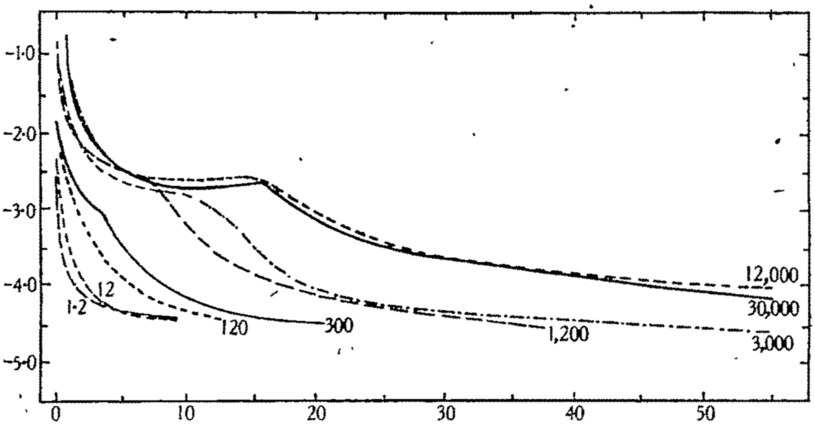

Fig. 7. Dark adaptation curves taken after various bright-adapting illuminations.

10 min. in full summer sunlight, followed by 5 min. gazing steadily at the clear sky. Thus it appears that the 12,000 e.f.c. field given by this apparatus, exposed for 3 min., is sufficient to outweigh any light adaptation which the subject is likely to have experienced in the ordinary course of events. The ability to delete the effects of previous light adaptation in this way is important in clinical investigations of individual variations in dark adaptation.

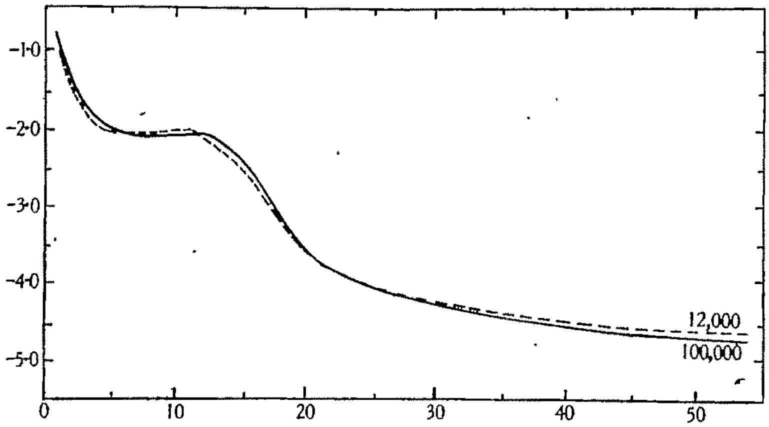

Fig. 8. Dark adaptation curves taken after bright-adaptation to 12,000 and 100,000 e.f.c.

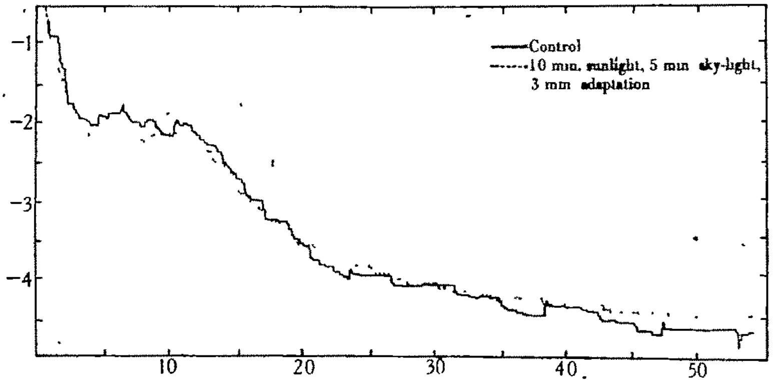

Fig. 9. Dark adaptation curves taken after bright adaptation to sunlight and to 12,000 e.f.c. (Unsmoothed, to show curves as recorded on drum.)

Dark adaptation curves on one subject, taken with 17° and 10′ fields, are shown in Fig. 10 and Table 3. Similar results were obtained on the other subjects.

Table 3. *Threshold in log e.f.c.*

Time in dark min.	Subject A			Subject B			Mean		
	Large	Small	Diff.	Large	Small	Diff.	Large	Small	Diff.
1	−1·7	0	−1·7	−1·5	−0·1	−1·4	−1·6	0	−1·6
5	−2·5	−0·5	−2·0	−2·5	−0·3	−2·2	−2·5	−0·4	−2·1
25	−4·6	−1·5	−3·1	−4·0	−1·0	−3·0	−4·3	−1·3	−3·0

Difference between threshold at 1 min. and at 5 min. with small field = −0·4, and between 1 min. and 15 min. with small field = −1·3. Ditto with large field, another −0·4 and −1·4.

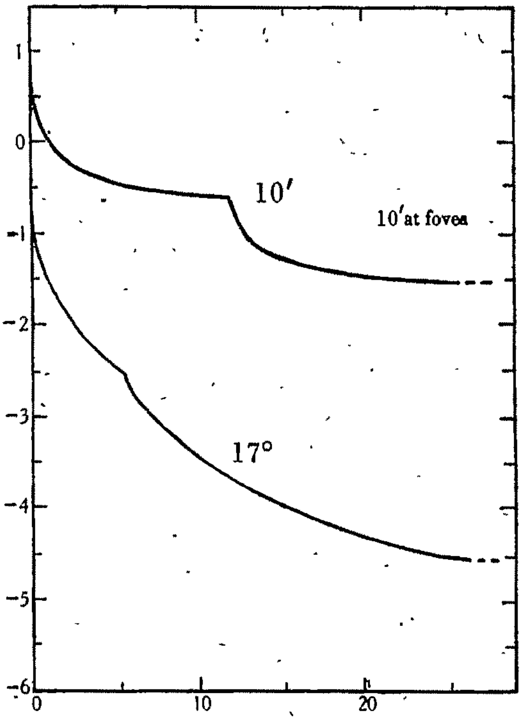

Fig. 10. Dark adaptation curves taken with 10′ and 17° test fields.

IV. Discussion

(1) *Effect of Variation in Bright Adapting Intensity*

Hecht, Haig & Chase (1937) obtained a family of curves showing progressively slower recovery with higher illuminations. They noted a tendency for the delay in recovery to become somewhat less at the highest intensities but concluded in general that "apparently above a light adaptation intensity of 20,000 photons" (about 2000 e.f.c.) "the subsequent dark adaptation curve has a fixed shape but a variable position on the time axis." Our present results show that, for bright adapting intensities above 12,000 e.f.c., the position on the time axis is fixed, if one disregards the form of the curve during the first half minute of dark adaptation.

The constancy of the time required for dark-adaptation with adapting illuminations above 12,000 e.f.c. seems at first paradoxical, since there is evidence that perfect bright adaptation to these higher illuminations occurs—i.e. that an eye exposed to 75,000 e.f.c. is 10/75 times as sensitive as it is when adapted to 10,000 e.f.c., so that the adapted subjective brightness is identical in the two cases (Craik, 1940). Yet the result fits easily into a photochemical theory of adaptation. The rate of dark adaptation is supposed (Hecht, 1926–7) to be determined by the concentration of photoproduct present at each moment. Now, if the eye has been adapted to an illumination so high that almost all the visual purple has been turned into photoproduct then adaptation to a higher illumination, though reducing the visual purple concentration at equilibrium still further (probably in direct proportion to the adapting light intensity), will add negligibly to the concentration of photoproduct and hence make no measurable alteration in the rate of regeneration or of dark adaptation. Thus, to take an imaginary case, if we suppose that in an eye adapted to 10,000 e.f.c., 99 % of the visual purple is in the form of photoproduct and 1 % remains as visual purple, then adaptation to 100,000 e.f.c. will reduce the visual purple concentration to perhaps 0·1 %. This increases the photoproduct concentration only to 99·9 %, which is practically the same as before.

At bright adapting intensities below 10,000 e.f.c. the recovery, both of rods and cones, is more rapid. The most reasonable suggestion is that of Hecht, Haig & Chase, namely that some intermediate photoproduct is formed by these less intense illuminations and that more rapid regeneration takes place from it than from the final product (possibly vitamin A in the case of visual purple). Although positive after-images are often quite marked after these lower illuminations, it would appear, as suggested in Part II, that they cannot account, by their masking effect, for the whole of this threshold rise from which the eye recovers so rapidly; some true reduction in sensitivity, neural or photochemical, must be postulated in addition.

(2) *Dark Adaptation with* 10′ *Field*

The difficulty of finding any photochemical substance in retinal cones is usually attributed to the much greater dilution of the cone substance which is thought likely in view of the 500-fold inferiority of the cones as compared with the rods in absolute sensitivity. This result, however, is obtained with large test fields subtending 5° to 15°, and may be due in large part to the greater summation occurring between rods than among cones. Stimulation with a small point of light should almost eliminate such summation and show more nearly the true ratio of rod to cone sensitivity. Hecht, Haig & Wald (1935) measured dark adaptation with fields as small as 2° in diameter, but even here much summation may be occurring, and the 10′ field used in the present investigation was considered preferable.

The results show that after 5 min. in the dark, photochemical sensitivity of the retina, as measured with the small field, appears to have increased only about 5-fold, and 20-fold in 25 min., as compared with its value after 1 min. in the dark. On the other hand, summation increases its sensitivity 20-fold in 5 min. and 200-fold in 25 min. Since the amount of summation increases with time we must suppose that dark adaptation consists partly in an increase in the amount of interaction in the retina with time in the dark, as suggested by Lythgoe (1940), if we assume that the visual purple and photoproducts are rigidly confined within the rods. Hartline (1940) has given electrophysiological evidence (in the frog) of the excitation of a single optic nerve fibre by illumination of several points on the retina and, conversely, of the excitation of several nerve fibres by illumination of a single small area of the retina.

V. SUMMARY

The progressive delay of dark adaptation with increasing bright adapting illuminations has been found to cease at approximately 12,000 e.f.c. This result agrees with photochemical theories which postulate that the rate of dark adaptation is determined by photoproduct concentration since the latter will be almost constant at high adapting illuminations.

Dark adaptation curves have also been taken with a test field subtending 10′. This almost eliminated summation among rods, since few were stimulated. Under these conditions rod vision is only about eight times more sensitive than cone vision, instead of about 100 times as with larger fields. The ratio of the thresholds with large and small fields also increases with time, suggesting an increase of summation during dark adaptation.

Our acknowledgements are due to Professor Bartlett for permission to work in the Cambridge Psychological Laboratory, and to the Medical Research Council for personal and apparatus grants for research on dark adaptation. We are indebted to Flying Officer Honeyman for assistance in the ophthalmoscopic examination of the eye under pressure.

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SPEECH IN AN IMPERFECTLY LEARNT FOREIGN LANGUAGE

By J. CREWDSON

From the Cambridge Psychological Laboratory

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  (4) *General difficulties of structure and content, as described in introspection* (pp. 91–93).
  (5) *General characteristics of language construction* (pp. 93–95).
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I. INTRODUCTION

WHEN a person, imperfectly acquainted with a foreign language, endeavours to speak in that language, certain deficiencies in his performance are easily observed. Some of these he could not avoid, for they result simply from his incomplete knowledge and experience. But it is characteristic that the general level of performance depends to a considerable extent upon the circumstances and conditions in which expression is attempted. So much is a matter of common experience. It is easier to hold a conversation with one person than to join in one with several. Embarrassment or a sense of urgency may limit utterance to an extent not justified by the poor status of the speaker's French or German. Conversation is easiest when it flows in comfortable social circumstances, and its object is the exchange of attitudes rather than of information. It is most difficult when the occasion demands the expression of precise information or the unravelling of complex circumstance. Fatigue will sometimes render almost speechless one who a few hours before was conversing fluently, and before it has taken full toll of performance, will induce progressive diminution in elegant and sophisticated modes of expression, together with increased tendencies to circumlocution and an incapacity for abstract transactions.

The existence of this fluctuation of performance, dependent on the one hand on the demands made upon the speaker's skill by the type of material he endeavours to express, and on the other by the circumstances in which he tries to express it, offers the possibility of approaches both to the problems of acquiring foreign tongues, and to the problems of language and its disorders in general.

Franz (1) pointed out certain analogies between the performance of the aphasic and that of a person trying to speak a foreign language, and no one who has had experience of aphasia, and has tried to converse in a language not his own, can doubt that the connexion is not fortuitous. In both cases there is an attempt at expression in the face of constraint, in the one case due to disease, in the other to imperfect acquaintance with the language in question. In both cases circumstances and the type of material to be expressed exercise a profound effect upon performance. I shall suggest that there are closer resemblances than these.

The investigation to be described is simple in method, and its scope is not wide, but the results suggest that further experiments along the same lines might be profitable. Two French children, aged eleven and fourteen years (both girls), who were able to speak some English, read prose passages in English, recalled these in English, and attempted to describe pictures shown to them. The linguistic errors were recorded, and are described.

II. MATERIAL

Two series of English prose passages were used, each about 140 words in length, and each series consisting of five passages of varying difficulty. In choosing the passages the factors taken into consideration were:

(*a*) Relative familiarity or unfamiliarity of words.

(*b*) Length of sentence (number of clauses contained in each).

(*c*) Relative simplicity or complexity of style (e.g. A 1 is simple, A 3 relatively complex).

(*d*) Subject-matter—abstract, descriptive or narrative.

(*e*) Relative proportion in which different parts of speech occur in the passage. (The fewer the nouns, and the greater the number of adverbial expressions, prepositions and pronouns, the more difficult the passage tends to become.)

The third series used consisted of four drawings of varying complexity, complexity having reference to the idea or situation expressed by the picture.

III. EXPERIMENTAL METHOD

Passages of Series A were presented singly to the subject, who was instructed to read the passage aloud to the experimenter. Having done this, the subject was allowed to re-read the passage silently, twice through if necessary, and then asked to state its contents in English to the experimenter, who took down as nearly as possible an exact account of what was said.

Series B was presented to the subject in exactly the same manner, but the subject, having read the passage aloud, was asked to re-read it aloud with a view to recalling it to the experimenter subsequently in French.

Series C, consisting of drawings, was presented singly to the subject, who had to describe what she saw. When she had done this, the drawing was removed, and she was asked to describe verbally in English what she remembered of the picture.

The subjects were on holiday at the time of the experiments. In order to secure full co-operation and attention, it was found advisable not to give them more than one passage and one drawing a day. The experimenter was well known to the children, and after the first day both appeared to be thoroughly at ease in the experimental situation.

IV. ANALYSIS OF MATERIAL

Series A. A 1. A descriptive prose passage entitled "At the Seaside". It contains familiar words, simple constructions and a familiar theme. (The children were at the seaside at the time of the experiment.) The style is extremely simple—it might be described as 'childish'. The verbal content is mainly nouns, verbs in simple tenses, and adjectives. It contains very few adverbial expressions, and only the very simplest concepts of spatial and temporal relations, e.g. "a castle with a wall all round it" and "until the sea came and washed it away".

A 2. "A Fairy Story" is a narrative prose passage with a connected theme running through it. The words are mainly familiar, except for one or two such as "wedding", "flattered" and "therefore". Some of the sentences in A 2 are longer than in A 1, containing up to three clauses. Spatial and temporal relations are still extremely simple.

A 3. "The Black Road" has a less concrete theme. It is mainly descriptive, but an intellectual idea runs through it, which depends for recognition on a grasp of the passage as a whole. It contains some words of a less familiar type, such as "errand of mercy", "starred", "christenings". The sentences are of varied lengths, the longest containing four clauses. The style is flowing and more artistic than that of A 1 and A 2.

A 4. "Death among the Poor" contains description of action. The difficulty of this passage lies mainly in the length of the sentences. The whole passage only contains five sentences, the longest of which has six clauses. The words are relatively familiar, except for "undertaken". The style is straightforward and unadorned. Understanding of this passage, however, involves appreciation of certain more subtle relational concepts, both temporal and directional, such as "it was no longer light", "hardly visible" and "through the passage".

A 5. "Hypnotism" has an abstract theme of a rather documentary type. The words are mainly familiar, except for "perform", "anaesthetics" and "soothed". The sentences are also comparatively simple, there being only one of more than two clauses. The style is heavy, and there is no definite connecting thread running through the passage. A certain grasp of temporal relations is involved in the phrase "Since then...". The difficulty of the passage lies rather in the ideas than in the words or style.

Series B. B 1. "A Large Farmhouse" is a simple descriptive passage, but containing slightly longer sentences and more nouns than A 1.

B 2. "Robin Hood" is a narrative passage, containing a relatively large number of nouns. Certain words, such as "bow", "quiver", "*provide* for himself", "*slung* on his back" and "summon" are unfamiliar, but the rest are fairly common. The sentences are longer than in B 1, but do not exceed three clauses. The style is straightforward, but not as simple as that of B 1. The only temporal relationship involved is in the sentence referring to Robin Hood's uncle.

B 3. In this passage, "A Dream", there is about the same proportion of familiar to unfamiliar words as in B 2. "Clutched", "growled", "bed-post", "toss", and "sheet" are fairly unfamiliar. Like B 2, it is a narrative passage, but with a rather unusual theme. The sentences contain mostly from two to three clauses, but the construction is complicated by the fact that action and simple description are intermingled, with a resulting mixture of tenses. Certain sentences involve complicated temporal relations, e.g. "M was just wondering what could have happened when...." The style of the passage itself is simple.

B 4. "Heroism of a Miner" is a detailed narrative passage. It contains a large number of words, of which a few are unfamiliar: e.g. "shaft", "kindle", "accordingly" and "blasting". The construction is complicated, with many sentences of three clauses, and several of four, and many additional phrases such as "leaving him to his fate...", "taking two stones...", "horrible to relate...". One sentence was complicated by having an adverbial phrase in the place of subject, namely, "One at a time was all the assistant at the top could manage". The difficulty of the passage lies mainly in the length of the sentences. Also, recognition of both spatial and temporal relations is involved in understanding the passage, i.e. that the men had to be pulled up one at a time, and that one was to be drawn up to the top, while the other was still at the bottom of the pit.

B 5. "The Effect of a London Fog" is a descriptive passage of an abstract nature. The words are familiar. The sentences are from two to four clauses long, but the constructions are difficult, and a grasp of temporal relationships is essential. Certain events occur during a fog, some after it, and one reference is made to events which had happened some time previously. There is no definite connecting thread running through the passage, but a general idea underlies it, namely, that a minor misfortune has the paradoxical effect of making people cheerful, and this idea must be grasped in order to understand the passage as a whole.

Series C. C 1 is a woodcut of a man leaning over a gate looking at a view of fields, haystacks, a farm cart, and farm buildings. That is the whole situation. In giving an account, nothing but a simple description is needed.

C 2 is a simple line-drawing from *Punch* of a portly butler bending down to turn the key in the keyhole of a door, holding a candle in his other hand. The picture suggests several possible situations. The most likely are (*a*) that the butler is going down to the wine cellar after the household has gone to bed, and (*b*) that he is locking up the front door before retiring himself.

C 3 is a humorous strip-cartoon from *Punch* entailing description of certain actions, situations, and also of the feelings and motives of the persons concerned.

C 4 is a political cartoon of a more subtle kind. It represents the Drake-Armada situation of modern politics. Chamberlain, dressed in Elizabethan clothes, is playing bowls while bombers bomb our warships out to sea. Unfortunately, neither the humour, nor the significance of the picture, were recognized by the subjects, and its value as representing a rather involved situation was therefore lost.

V. TREATMENT OF RESULTS.

The experimental results took three forms:

(*a*) Records of what was actually said in response to the task.
(*b*) Notes of general behaviour and speech characteristics.
(*c*) Introspections.

(*a*) Records of the subjects' remembered account of a passage or picture were not always as accurate and comprehensive as was desirable. The subject often spoke too fast for her account to be taken down word for word. Errors of pronunciation and construction, however, were noted as carefully as possible.

(*b*) In describing general behaviour, and the characteristics of speech, a scheme was drawn up and a note made indicating to what extent each given characteristic was present or absent from the record. Thus notes on general behaviour referred to (1) *general expression of the face* (whether strained, smiling, etc.), (2) *movement of the mouth and lips* (whether exaggerated for the production of sound, (3) *general posture* (whether strained or fidgety, etc., hands). These characteristics remained fairly constant on the whole.

With regard to the characteristics of speech, the more general characteristics were found by the experimenter to remain fairly constant throughout the whole series of experiments. These were *pitch of voice, intonation* and *loudness*. *Speed of utterance* also appeared to remain fairly constant, but unfortunately the experimenter was unable to make an accurate measure of speed, since she had no stop-watch, and the variation in length of time taken to read a passage could never have been more than a few seconds.

Other, more specific, characteristics varied a great deal, and appeared to be more significant as indications of the ease or difficulty of a given passage. These were:

(a) General rhythm of sentence, whether matched to sense and syntax or not.
(b) Tendency to slur words.
(c) Tendency to contract.
(d) Hesitation.
(e) Alterations.
(f) Repetitions.
(g) Stuttering.
(h) Mispronunciation.
(i) Types of grammatical error.
(j) Tendency to simplification.

The experimenter was again handicapped to a certain extent in recording the above characteristics, since she was naturally unable to direct her attention equally to all aspects of the subject's speech and behaviour at once, and minor differences in pronunciation were sometimes unavoidably overlooked.

(c) *Introspections*. An attempt was made to obtain full introspective reports from the subjects after each passage. This included an account of any visual imagery the subject might have had, any unfamiliar words or difficult constructions, any tendency to translate, or to remember certain words and phrases rather than others.

VI. RESULTS

(1) *Variations in general behaviour*

The general behaviour of the subjects varied little. The only signs of strain which were elicited under specially difficult conditions were fidgety movements of the *hands*, and a worried *facial expression*. This occurred usually when the subject was trying to remember something. She also sometimes looked round the room with an expression of concentration, but for the most part she looked either at the table or at the experimenter. Her *expression* was usually serious or pleasantly interested in the task. *Lip movements* were consistently slightly exaggerated by English standards, but they did not vary noticeably except in the case of Subject 1, who sometimes exaggerated *h* sounds considerably, protruding her lips, opening her mouth wide, and taking a deep breath. This was due to a natural difficulty in sounding *h* at all, and occurred whenever the subject's attention was anxiously directed to sounding her *h*'s. B 2 provides a good example in which the subject's attention was "*h*-directed" throughout. Not a single *h* was dropped, and none were over-exaggerated. The same thing occurred in this passage with Subject 2. Subject 2 dropped a large number of *h*'s in the first reading, ending with "...while the captain always 'ad a 'orn with 'im, to summon 'is followers about 'im...". At the second reading, however, she became *h*-directed, and over-emphasized seven *h*'s, dropped none, and inserted two extra ones—namely "*h*into" and "*h*outlaws".

(2) *General speech characteristics*

Both subjects spoke in a very monotonous, ***high-pitched voice***, which never varied. There was a complete *lack of intonation*, and each passage was read throughout at an even, high pitch, with no variation of loudness, and no light and shade. The *speed of reading* was also fairly constant, no noticeable variation taking place, but it was impossible to measure this factor accurately. *Hesitation* occurred frequently, and *repetition of words* less frequently. This was sometimes due to a word being unfamiliar, or to the subject not knowing how to pronounce it. At other times it was due to the length of the sentence or the complexity of structure, and at other times, again, no definite reason could be put forward to explain it. *Stuttering* appeared to occur for the same reasons, namely, unfamiliarity of words or constructions.

The *general rhythm of speech* was jerky. In the easier passages, as might be expected, the sentences ran more smoothly than in the harder ones. In the case of Subject 1, A 1 and B 1 were read extremely smoothly the first time, with no gaps in the rhythm. At the second reading of B 1, however, the subject's attention was directed to the sense of the passage, and the rhythm became broken up in different places. In the harder passages, however, there was a tendency for the rhythm to improve slightly at the second reading, which indicated that familiarity with the material was a more important factor than attention, once a certain degree of difficulty had been reached. This tendency is shown in Table 1.

There is a certain amount of correlation between the number of breaks in rhythm and the difficulty of the passage in the case of Subject 1. Subject 2, however, shows no significant tendency in this direction. Although the sense of a sentence was sometimes ignored by breaks in the rhythm, the rhythm always fitted in with the syntax. That is to say, full stops and other punctuation signs were always marked by small breaks in the rhythm, although there was no change in the intonation.

Table 1. *Number of breaks in rhythm*

	Subject 1		Subject 2	
	1st reading	2nd reading	1st reading	2nd reading
Series A 1	0	—	3	—
2	3	—	0	—
3	7	—	5	—
4	10	—	10	—
5	5	—	6	—
Series B 1	0	5	1	3
2	4	5	12	13
3	10	4	0	0
4	17	9	5	7
5	13	4	12	12

(3) *Special speech characteristics*

A certain number of characteristic faults of pronunciation tended to appear during speech. These are:

(1) *Th*-sounds pronounced *s* or *f*.
(2) *h*-sounds dropped.
(3) Vowel sounds changed or modified.
(4) Mute syllables sounded.
(5) End syllables left unsounded.
(6) Contraction of syllables.
(7) Wrong emphasis.

The most common faults were the first two, which seemed to give a better indication of difficulty in a passage than any other characteristic, since subjects were conscious of their tendency to drop *h*'s and mispronounce *th*'s, and were sometimes able to eliminate these errors by conscious effort. Thus they tended to lapse in these respects whenever a passage presented particular difficulties. A good example of this is shown by Subject 2 in B 5. The first two sentences were read correctly. Both the second and the third, however, were particularly difficult, and this caused her to say—"One at a time was all *se* assistant at *se* top could manage." Two more *th*'s were turned to *s*'s farther on in the passage, where the subject afterwards reported great difficulty, and once more at the end, at another complicated point.

Mispronunciation of vowel sounds also occurred fairly frequently. This was sometimes due to ignorance of the right pronunciation, but discounting these cases, there seemed to be a slight tendency for the number of mispronounced vowel sounds to correlate with the difficulty of the passage. A table is given (Table 2) showing the number of these mispronunciations occurring during the first reading of Series A and B for both subjects. Vowel sounds were mispronounced far more frequently during reading than during spontaneous speech. In the latter it only occurred where the words were unfamiliar, and the subject was uncertain of the correct pronunciation.

Table 2

	A 1	A 2	A 3	A 4	A 5	B 1	B 2	B 3	B 4	B 5
Subject 1	1	3	3	4	4	3	2	4	5	8
Subject 2	1	5	2	5	5	4	4	—	8	9

Mispronunciation took various forms:

(1) Vowel sounds were sometimes completely changed: e.g. *en*tered to *on*tered (A 4, S 1), *ea*sy to *a*sy (A 3, S 1), gre*a*t to gre*e*t (A 2, S 1).

(2) More often a short vowel was lengthened: e.g. g*i*ve to gīve (A 2, S 1), me*a*dow to me*e*dow (A 1, S 2).

(3) Or a long vowel was shortened: e.g. be*a*st to b*e*st (A 3, S 2), c*a*me to c*a*m (A 1, S 2).

(4) A mute syllable might be pronounced: e.g. excit*e*ment to excit*er*ment (B 3, S 1), soothed to sooth-*ed* (B 3, S 1).

(5) A final syllable might be omitted: e.g. complet*ed* to complet/ (B 5, S 2).

(6) Very occasionally syllables were contracted: e.g. happ*i*ness to happ'ness (B 3, S 1), accord*ingly* to accord*ling* (B 4, S 1), cur*ious* to cur*os* (B 3, S 1).

Another characteristic of mispronunciation was *wrong emphasis*. This occurred comparatively frequently, but was only slightly more frequent in reading than in spontaneous speech. The experimenter was inclined to believe that errors of emphasis varied less than other characteristics with changes in the difficulty of material. It appeared rather to be a constant characteristic of speech due directly to the influence of the French language. For example, a lingering emphasis occurred frequently in the last syllable of a word, e.g. mast*er*, weep*ing*, hand*some*, short*er*. This is a common characteristic of French pronunciation. Secondly, wrong emphasis occasionally occurred in the middle of words, e.g. veh*e*mently, imagin*a*ble. This was almost certainly due to similar emphasis in the corresponding French words. Finally, as Table 3 below shows, there was no tendency for the number of wrongly emphasized

Table 3. *Number of wrongly emphasized words in a passage*

	A 1	A 2	A 3	A 4	A 5	B 1	B 2	B 3	B 4	B 5
Subject 1	2	4	1	5	1	1	3	7	4	0
Subject 2	0	4	1	0	0	1	3	0	6	0

words to correlate with the order of difficulty of the passage. This supports the suggestion that wrong emphasis is a fairly constant characteristic of speech under these conditions, and that its occurrence is not affected significantly by the difficulty of a passage.

(4) *General difficulties of structure and content, as described in introspection*

(1) *Length of sentence.* The factor which caused both the subjects the greatest difficulty was length of sentence, or number of words in a phrase. They frequently complained during introspection that there were "too many words", that they knew them singly but could not understand what they all meant together. B 4, for instance, contained a great many sentences which troubled the subjects in this way:

> "One at a time was all the assistant at the top could manage."
> "Taking two stones, a flat and a sharp, he succeeded in cutting it to the right length."

The whole of the last sentence, from "Both shouted..." gave an impression of confusion, but this is not only due to the length of the sentence, but also to the fact that the nouns are replaced by "both", "he" and "them". This latter factor will be referred to later.

(2) *Complicated verb structure.* Long verbs were another source of difficulty to both subjects. However familiar the verbs were in simple tense form, they tended to lose all significance when met with in a compound tense, or in conjunction with another verb.

E.g. S 1, B 3. "One night he could not go to sleep."
("Couldn't grasp the meaning of this.")
S 1, B 3. "...was just wondering what could have happened."
S 2, B 2. "His uncle had been persuaded to leave his property...."
("Forgot the meaning after reading it.")

One result of both these complicating effects was that the subjects were unable to remember the beginning of the sentence by the time they had reached the end. In B 5, for example, the last sentence, beginning "One reason is...and the other is..." was rapidly forgotten, although each bit was understood at the time of reading. Similarly in B 3, S 1, "He tossed about and woke his brother John, who growled at him drowsily." ("I couldn't remember the sense of this after I had read it.")

(3) *Temporal and spatial relationships.* Expressions involving temporal relationships presented great difficulty to both subjects. This, no

doubt, was an important reason for the confusion caused by complicated verb structures.

> E.g. B 2. "His uncle had been persuaded...."
> B 3. "...was wondering what could have happened."
> A 5. "He will perform actions long after he has awoken from his hypnotic sleep."

Spatial relations of any degree of complexity also caused confusion.

> E.g. A 4. "...so creeping cautiously through the dark passage and bidding Oliver keep close to him...." ("Got involved in that. It is very complicated.")

B 4 also demanded a grasp of spatial relationships, namely those of the positions of the two miners and the assistant. But it is significant that neither subject was able to give a coherent account of the situation.

(4) *Isolated unknown words.* A less frequent source of difficulty was that of meeting single words whose meanings are unknown. Normally this did not affect the subject much, as long as the word was not essential to the general sense, but occasionally one strange word affected understanding of the whole sentence. This happened to S 1 in B 5. She did not know the meaning of "accordingly" in "accordingly tried to break it shorter"; and this affected her so much that she was unable to understand the rest of the sentence.

(5) *Abstract ideas.* Passages containing abstract ideas appeared to be more difficult than descriptive or narrative passages. Both S 1 and S 2 complained that the "ideas were difficult" in A 5 and B 5, and S 1 said that the sentences were disjointed and "décousu"!

(6) *Lack of nouns.* Owing to the ease with which both subjects tended to forget what has gone before, and even the beginning of the sentence they were just reading, they naturally tended to cling to those words which recall the subject-matter of the sentence, and the general sense of the whole passage. Since it was mainly nouns which had this property, passages containing relatively few nouns were more difficult than others. Thus passages containing a large number of pronouns and relative adjectives, which have no meaning in themselves, caused the subject great confusion. E.g. B 5, "Both shouted vehemently to the assistant; both sprang to the basket, but he could not lift them both at once." This sentence contains only two nouns "assistant" and "basket". The sentence was found to be very complicated by both subjects, who

could not even understand the general sense, although all the words were familiar to them.

(5) *General characteristics of language construction*

As might be expected, all those factors which cause the subject any difficulty in reading prose are eliminated as far as possible in spontaneous speech, and where this is impossible, grave difficulty is encountered. The spontaneous speech of the subjects was characterized in general by the following tendencies:

1. Simple sentences, containing rarely more than one subordinate clause.
2. Simple verbal constructions. (*To be* is used as far as possible.)
3. Frequent disturbance of *th* and *h* sounds.
4. Great difficulty in expressing temporal and spatial relations.
5. Repetition of nouns, and avoidance of pronouns.
6. Tendency to translate literally, or to repeat parrot-like.

The above tendencies can best be observed by reading the actual reports made by the subjects in Series C, and in the second half of Series A. As these show, the longest sentences contained not more than three clauses, of which one is almost invariably connected by "and". More than one relative clause never appears at a time. Verbs present peculiar difficulty. The verb *to be* is used whenever possible, and this is usually used correctly, but any other verb tends to be either in the wrong tense or the wrong person. Mistakes of this kind are not simply due to ignorance, since the experimenter was satisfied that verbs could be correctly used in ordinary conversation. The subjects often dropped the final *s*, as in C 3, S 2: "The osser Mister look/ at se first", or as in C 4, "put/", "fall/", "go/", "open/", for the corresponding third person singular forms. Similar errors of number are occasionally made in nouns, such as "pupil/" for "pupils" in C 4.

Errors of *th* and *h* pronunciation occur relatively more frequently in spontaneous speech than in reading. Subject 2 was relatively worse than Subject 1 in this respect, which is interesting, since they had approximately equal scores of 51 and 49 over the whole of Series A and B, but in Series C Subject 1 had 22, and Subject 2, 58. This can probably be explained by the fact that Subject 1 was older than Subject 2, and had had more practice in talking English with the experimenter. She was consequently more familiar with the "speaking situation" although her actual standard of expression was no better than that of Subject 2.

Considerable difficulty was found by both subjects in expressing temporal or spatial relationships. Thus in C 4, Subject 2, unable to say that the pot of water fell on the boy's head, said instead: "A pot of water hit on him" in C 4, Subject 1 said "...and they are before the water..." meaning "by the sea". In the same passage the same subject said "...and turn the back of the sea..." meaning "turns his back to the sea". In A 1 she said "all around the wall," meaning "with a wall all round it"!

Temporal relations were almost entirely ignored and prepositions continually misused. "At" was used for "to", "on" for "in". We find "shoot *in* the boat" for "shoot *at* the boat", "*on* a wall" for "*against* a wall"; "*in* a hill" for "*on* a hill".

When the subject was in difficulties she also tended to drop articles, e.g. "I see farmer on his cart with black horse" (C 1). The same difficulty, caused by lack of nouns in prose passages, appeared for Subject 1 in spontaneous speech. She reported a tendency to forget what she was talking about in describing a picture, and this led her to repeat nouns instead of using pronouns, as a reminder of what she had just said. E.g. in C 2, she reported "When I have said one sentence, I pay attention to the next, and forget what I have just said, so I must repeat the word to remind me." E.g. "This man" instead of "He." "If I use the pronoun I get mixed up between people and objects". Similarly in C 3, she constantly repeated "the pupils", "the basket of books", "the master", instead of using the relative pronoun. C 1 illustrates this repetition admirably: "This farmer reaches his farm", "This farm was...", "This farmer looked at...", "this farmer smoked...". She was aware of the repetition, and said "I could not link my ideas up properly, which is why I repeated the noun".

A second aid adopted by Subject 1 to help her to remember what she wanted to say was to decide what she wanted to say in French, and translate it literally into English. She did this also when describing a picture from memory, instead of attempting to remember her previous description in English. Thus in C 2 she said, "He bears a (candle) in the left hand, and he leans to see the aperture". She knew the words, "carry", "bend" and "hole", but she felt compelled to keep the words as close to the French as possible.

In describing Series A from memory, Subject 1's methods were different. She relied almost entirely on her memory of the words and constructions used in the passage. When this broke down, she was more at a loss than was Subject 2, who relied less on memory of words, and more on memory of sense. A good example of the difference in method is shown by comparing the reports of the two subjects in A 3. The following is an account of Subject 1's report of the passage, leaving out mispronunciations, repetitions and gaps:

> The road was built long ago for the man and beast. It was a pleasant windy ribbon. The banks on either side—starry—flowers—unimaginable—and deep in grass. On the road there were christenings, funerals, weddings. The doctor go by on his errand, the farmer on his car, Madam on her carriage. Nowadays—unaccustomed—dare be free—to him.

In her introspection she said, "I relied on my memory of individual sentences. Did not bother about general sense in describing it." Subject 2 said:

> The road was then large, and easy for the feet of the travellers and the bare feet of the children. On every side there was flowers—no—all imaginable flowers. It meant weddings, christenings, funerals, the doctor in his carriage, farmer in his cart. Now the road belongs to the motorist, and it is rugged to unaccustomed feet.

Afterwards she said, "I tried to remember the sense rather than the words, but had to learn two words I didn't know". She left out the sentences she did not understand.

It seems unlikely that this difference in method could be due to a difference in knowledge of English. Subject 2 was two years younger than Subject 1, had done a year's less English, and had a slightly smaller vocabulary. On the other hand, judged on a basis of school achievement and general observation of the experimenter, Subject 2 appeared considerably more intelligent than Subject 1, and it seems probable that the method adopted by the former in remembering and describing A 1 depended on her superior intelligence rather than on the degree of knowledge she possessed.

(6) *Avoidance of abstract situations and description of concrete details*

One marked tendency which was evident in the results of the Series C experiment was the avoidance of any attempt to describe situations, ideas, or motives. Both subjects directed their attention wholly to description of concrete details and objects.

Series C 1 to C 4 are arranged roughly in order, starting with complete absence of underlying situations or ideas, and ending with a picture in which the idea is all-important, and the details insignificant.

In C 1, neither subject attempted to describe anything that was not there in the picture. In C 2, Subject 1 described, the first time, only what she saw in the picture, but in the account from memory, she attempted to supply a motive by saying "to fetch perhaps a bottle of wine". Subject 2 merely described the bare details.

C 3 provides more opportunities for discussion of motives, ideas, etc. Subject 1 made a valiant attempt to express the situation by saying "two pupils have imagined to put in a basket many books and to make a trick to the master". Farther on she noted that the pupils laughed, and the master was angry. But this again is only describing what she actually saw in the pictures. She made no attempt to explain why the headmaster called the boys to his study, nor that it was he who placed the jar of water on the door to get his revenge. Subject 2 never rose above the level of concrete description throughout. She made no mention of motives or attitudes on the part of the master or pupils.

Unfortunately C 4 was badly chosen by the experimenter since the children neither saw the reference to Drake and the Armada, nor the significance of the picture in connexion with current politics in England. No attempt was made therefore to describe the idea underlying the picture. This cannot be attributed to the language constraint, but was simply due to lack of comprehension. Neither subject attempted to give the picture any meaning at all, but Subject 2 remarked that "It is funny, for there were no aeroplanes in the mid-age".

(7) *Greater ease of description of picture when remembered than when present*

A final point of interest in connexion with the results of Series C was the greater ease of description when the picture was not present. During introspection Subject 1 said several times "I found it easier to describe without the picture, because I could follow my ideas without having to attend to what I saw before me" (C 4).

This appeared to apply to both subjects, since reports from memory were almost universally more coherent and better constructed than descriptions of the actual picture. This is particularly apparent for Subject 1 in C 2 and C 3, and for Subject 2 in C 1.

VII. DISCUSSION AND CONCLUSIONS

Learning a language, like speaking a language already acquired, involves the double task of knowing the meaning of single words, and of using them in different contexts, which may alter their general sense and give them significance only in relation to the whole setting in which they stand. Thus the meaning of the sentence is prior to the meaning of the words composing it, and comprehension of a sentence or a paragraph depends upon the ability of the individual to treat the sentence as a unit of meaning and to relate one unit to another. This ability breaks down in the case of language constraint, and the results of our investigation give ample proof of it. Anything which affected the complexity of the sentence as a unit of meaning—number of words, complexity of construction, unknown words, words lacking intrinsic meaning-value (e.g. "he", "which"), degree of abstraction of the ideas expressed—all tended to create difficulties for the subject in her efforts to treat the sentence as a whole and to relate it to other wholes. The same difficulties appeared in spontaneous speech, where the subject was forced to adopt certain devices for remembering the whole sentence, such as repeating the noun, and translating from French, keeping as closely to the constructions and words of the latter as possible.

These results show a certain similarity to those of Oldfield & Zangwill in their experiment on memory (3). They concluded from their results that the first stage of learning is probably the formation of a general scheme incorporating some dominant detail. They suggest that intrinsic meaning is very important in scheme formation, and must be taken into account in any theory of remembering. It seems likely that the same principles apply in language learning. Introspective evidence shows that in order to reproduce a passage, the subject's main consideration was to get a general scheme or to grasp the general sense of the passage. When lack of knowledge or the abstract nature of the material interfered, the subjects frequently resorted to a "parrot" method of learning. Subject 1 often adopted this method, but Subject 2 attempted to form a general scheme wherever possible. This is of some interest since it shows that the material to be learnt does not entirely decide which method of remembering shall be adopted. Other things being equal, two subjects may yet adopt different methods of learning the same material. The experimenter is inclined to think, on the evidence obtained from Subjects 1 and 2, that this individual difference is probably due to some factor such as general intelligence.

Finally, the results indicate that the most important reaction tendency in verbal reproduction of material in a foreign language is reversion to a more primitive mode of reaction. Literal translation and dropping *h*'s are two of the most obvious types of reversion, and no doubt other characteristics of language construction and pronunciation are probably indirectly of the same nature.

The general similarity between the speech of an aphasic and the speech of one who is in the process of learning a foreign language has already been pointed out in the introduction, but there are certain specific tendencies which seem to indicate the fundamental similarity of the two types of "disorder".

The first and most striking of these is inability to express ideas of spatial and temporal relationship and to deal with ideas of any considerable complexity. This tendency to simplify ideas is a symptom of semantic aphasia. People suffering from this disorder are unable to comprehend the general significance of a set of details or the principles underlying a game etc. They talk rapidly as though they were in danger of forgetting what they wanted to say. In reading they tend to forget the beginning of a sentence before they reach the end, and therefore they cannot grasp the general significance of a story or a joke. In speech in a foreign language, the same defects are apparent, especially in relation to the comprehension of abstract ideas, or sentences involving a large number of words.

The other types of linguistic defect, the verbal syntactical and nominal, are also present to a certain degree in speech in an imperfectly learned foreign language. But they cannot easily be distinguished, since their occurrences are causally inter-related to one another and to the semantic type.

A second point in common between aphasic speech and speech in a foreign language is the effect which complexity of ideas or of logical structure may have on articulation and grammatical structure. The aphasic can often pronounce and articulate correctly only in simple circumstances. Where the problem he is posed is more complicated his articulatory processes tend to break down. An exactly similar effect is found in speech in a foreign language.

VIII. SUMMARY

An investigation was carried out with two French children of 11 and 14 years to study the ways in which speech is disturbed in a foreign language. The methods used were those of *Reading*, *Description* and *Recall*, the investigation being conducted in English. Certain types of breakdown were found in speech under these conditions which resemble defects found in aphasia. The chief of these were: (*a*) tendencies to simplification; (*b*) tendencies for difficulties in the material to be expressed to reflect upon the articulatory processes; (*c*) tendencies to revert to a more primitive mode of speech reaction; (*d*) tendencies to avoid abstract features of the material.

It is thought that the investigation suggests further lines of work which might be of importance to the study of aphasia, of the learning of foreign languages and of language generally.

The author wishes to express her thanks to Mr R. C. Oldfield for his assistance in planning this experiment, and for his advice in the discussion of the results.

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NOTICE TO CONTRIBUTORS TO THE BRITISH JOURNAL OF PSYCHOLOGY

It should be noted that both the time and the cost of publication of the *British Journal of Psychology* have been greatly increased recently on account of the war. Contributors are therefore asked to observe particularly the conditions laid down in the former Notice to Contributors (see this *Journal*, 1937, XXVII, 329). Copies of this Notice may be obtained from the Editor.

Contributors are reminded especially:

(1) To **keep their proof corrections to a minimum**; in future, lengthy alterations will be deleted.

(2) To **return their corrected proofs within the shortest possible time.**

PUBLICATIONS RECENTLY RECEIVED

Colour Co-ordination. By M. Sargent-Florence. London: The Bodley Head. 1940. Pp. 352.

The author is an artist who sets out to find "some working basis of agreement" between scientists and practical colorists in regard to colour theory. The book consists largely of a diffuse discussion of the musical analogy for the relation of the spectral colours, as developed by Newton, Ostwald, and others. Unfortunately, the writer does not make the contribution to the subject which an artist can and a scientist very often cannot—a significant and sympathetic statement of the colour relationships actually used by artists to express certain feelings or situations. A reliable discussion of such colour relationships and their meaning—an indication of those appositions of colour which are essential in a picture and those which are more or less coincidental and of those which satisfy and those which offend the trained artist—would be valuable to the psychologist or physicist. This book almost completely lacks such discussion. Almost the only actual colour situation discussed is that of a butcher's shop with red meat, buff paint, and the blue apron of the butcher—hardly a typical instance of art! The fundamental difficulty lies deeper, however. The author fails to show an understanding of the basic principles and purposes of scientific method, and for this reason is not likely to obtain the sympathy of the physicist. For instance, great pains are taken to defend the musical analogy for the spectrum against various specific objections of numerical dissimilarity; but there is a failure to see that the justification of hypotheses in science comes mainly from their scope—not from the ability of their proposers to defend them against each criticism in turn. The concept of the octave in music is accepted not just because it is an ingenious nomenclature, but because all European music is built on this scheme and the correct or incorrect sound of harmonies can be traced to the (conscious or unconscious) obedience or disobedience to its rules. Harmony is thus a set of laws applying, in a quite rigid manner, to a wide realm of facts; and in this it resembles a set of physical laws. But the mere numerical pattern, borrowed from music and applied to colour, accomplishes nothing. It is the propounding of laws which work, and nomenclature that suggests new facts, which constitutes science—not just the invention of a complicated terminology for its own sake. Yet it is this purpose of scientific nomenclature and hypothesis which the author seems throughout to ignore; one gets an impression that the task of colour science is accomplished if the musical analogy for colour can be made just 'water-tight'; the possibility that it should have a further purpose, and be justified by its accomplishments, seems never to occur.
K. J. W. C.

The Structure of Art. By Carl Thurston. University of Chicago Press. (Cambridge University Press). 1940. Pp. xiii + 190. 72 illustrations. 15*s*. net.

The great merit of this book is that it arouses a lively interest in the phenomenal nature of the complex aesthetic experience, and its relation on the one hand to the 'form' of the work of art, and on the other to its 'meaning' as based on the memories and personality tendencies of artist and observer. The author wisely limits his study to the 'spatial' arts; and in dealing with simple shapes, decorative art, architecture and sculpture, he successfully indicates some at least of the essential features in the relation of 'form' and 'meaning'. But he would be somewhat easier to follow—and his study would be more 'scientific'—if he included more specific detail and some actual experimental data relating to the phenomenal processes involved. The complexity of

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THE NATURE OF SHYNESS[1]

BY HILDE LEWINSKY

From the Department of Psychology, University of Manchester

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I. A SOCIAL PHENOMENON

THE phenomenon of shyness has received very little systematic attention (1,2), although it is of special interest in applied psychology, where tester and psychotherapist have to overcome shyness in order to obtain rapport.

Shyness is mainly a social phenomenon, always expressed by behaviour and only in relation to other human beings. A person alone on an island would not be called shy: we might speak of his timidity or modesty, but not of his shyness. The meaning of the word 'shy' is vague (cf. (3), p. 10). We find shyness described as a character trait, as an attitude, or as a state of inhibition. The fact of inhibition as such is not a distinguishing mark, as the inhibition of impulses is commonly expected after the earliest phases of childhood. However, inhibition in shyness is not volitional, but compulsory. It does not further either maturation or civilization, which are the aims of social inhibitions. It is a state of hyper-inhibition, usually accompanied by physical symptoms like blushing, stammering, perspiring, trembling, going pale, accessory movements and increased urinary and faecal urges. The mental state is described by the individual as a feeling of inferiority, of not being wanted, of intruding; it is coupled with an inability to say the right thing at the right moment—while a hundred good answers and quick retorts can be thought of afterwards. The individual feels over-conscious of himself, of his mental attitudes, his emotions, and especially of his appearance. A very striking feature of shyness is the wish to avoid company; the desire to be alone is counterbalanced by the ardent wish to make friends, to come into contact with others. This ambivalent attitude seems of great importance in the understanding of shyness. A further general complaint by the shy person is that he feels unable to show himself to advantage, to utter his opinion, whether friendly or hostile. From the wealth of feelings aroused within the shy person we can extract three main aspects: incapacity to make contact with others, inability to show oneself to advantage and dependence on the judgement of others.

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[1] Substance of a paper read to the British Psychological Society's Nottingham meeting on 20 April 1941. Part of a thesis on "Play and Therapy." accepted for M.Sc. in the University of Manchester.

II. THE MAIN FACTORS IN SHYNESS

Let us consider an instance of shyness: somebody in a strange town is too shy to ask his way. To simplify the example, we shall suppose that the individual has no unconscious resistance against his aim; e.g. sightseeing, and that he is not pressed for time. The ability to come into contact with a stranger is strongly inhibited. From this naturally is derived an incapacity to deal with the situation to the individual's manifest advantage. What particular features are implied in this contact with a stranger?

The shy individual must choose a particular person to approach, think of what to say and the right way to say it. This implies a confession of ignorance and a request for help. The basis of the whole contact is the individual's preparedness to trust and accept the help, if given. Which inhibiting factors make it difficult or impossible for the shy individual to perform these actions?

The three main factors are: *fear, shame* and *mistrust*. There is fear of being misunderstood in language or motive, of putting the request incorrectly or in an improper way, of appearing to molest, of becoming conspicuous. Especially strong is fear of ridicule—in case the place he asks about turns out to be 'just round the corner'. There is furthermore fear of refusal, of being 'snubbed'. The individual is ashamed of the actual fact of needing help, because this shows openly his dependence on others. It makes clear that he does not belong to the group, to this district, this town; that he is different, strange, an outsider. There is also shame of ignorance, of not knowing something which possibly may be quite obvious, and which everybody else seems to know. The third factor, mistrust, is directed both towards the informant and the questioner himself. He mistrusts his power to understand the directions, if given, and this would involve a further request for help and patience, and increase the feelings of shame.

All these factors—with the possible exception of having one's motive misunderstood—are regular inhibitory causes or stimulating influences upon a child's behaviour. The child's ignorance and curiosity often meet with amusement from adults; the wish for independence, for being grown up, the desire to be able to do things alone, the urge to be 'in the know', which may be connected with sexual curiosity, belong to a phase of childhood round about the third year. The experience of receiving evasive answers, of being snubbed for asking questions or for arguing, is common in the child's daily life. Children are also often warned not to accept anything from a stranger. We could therefore understand the complex if we were to meet it only in children, as the young child of the age of about three years does not want to make contact with others, especially during Piaget's so-called 'monologue-phase' (4); the child of this age is unable to compete with grown-up people who are stronger and cleverer than he, and he is certainly entirely dependent for his security on the judgement and good will of adults. This suggests that the adult who behaves in such a way possesses a weak and egocentric ego.

III. SHYNESS TOWARDS INDIVIDUALS AND TOWARDS GROUPS

Shyness occurs only in special circumstances. When we compare the situations in which the person 'apt to be shy' becomes shy, there are two types of circumstances which seem to provoke shyness: (1) situations in which the individual is expected to express in a responsible way his opinions or feelings, whether friendly or hostile, to stand up for himself, to assert himself, to 'show his hand', and (2) those situations where conformity with an objective standard is expected, e.g. in examinations. Both occasions could be called 'showing your hand', that is, first, expressing what you think, want, feel, etc., and secondly, submitting yourself to an objective judgement. This shyness depends less upon the actual difference (not necessarily only superiority) of the other person or group than upon the shy individual's premonition that he will be measured objectively. That, for example, the aspect of other people's superiority is mainly a cover for the expectation of criticism—whether silent or outspoken—is shown by the fact that many adolescents feel more at ease with a new group of older people than with a new group of contemporaries. We can assume that these older people will not view the youngster as objectively as would his equals in age. Children are accustomed to be judged not according to their actual work done or knowledge shown but with regard to their age level, and in the family circle this judgement is usually emotionally tinged. These instances suggest also that the shy individual has clung to his childishness and is not fully grown up.

As far as shyness towards groups is concerned, it appears that the individual wants to be recognized as a member of the group by its members. If actual recognition is not possible on account of internal or external differences, the individual may still wish to be regarded, by those outside or inside the desired group, 'as if' belonging to the group. The advantages of belonging to a group, especially one admired by the individual, are manifold. He can expect the objective evaluation by the group members towards him to be changed, e.g. he gets from the other members loyalty, privileges and help which decrease his tension and apprehension and make him feel secure. Membership of any group in authority, especially when manifested by a uniform—as the fighting services, police or even bus-conductors—neutralizes individual shyness. A policeman may be shy in private life, but when in uniform he is normally able to control his behaviour and not to appear shy. The uniform helps to attain a standard of objectivity otherwise not reached.

IV. SHYNESS AND BOASTFULNESS

When considering the psychology of shyness, we have to deal also with the contrasting phenomenon of boastfulness and bragging. The mental attitude which accompanies this form of behaviour appears to be similar to that in shyness. The outward form of 'blustering self-assertion', as McDougall (5) calls it, is described by many authors as an attempt to overcome one's shyness, to disguise it or to cover it up. In boastfulness as compared with shyness we find a change from passivity to activity; instead of withdrawing we meet forwardness, the aggressive character of which becomes very obvious. The hyper-inhibition of shyness is replaced by a lack of inhibition in boastfulness. The individual does not express feelings of inferiority but assumes an air of superiority. Both forms of social relationship have in common a feeling of great insecurity and the incapacity to show oneself to advantage. Although in boastfulness the contact with others is increased, the result remains mainly negative. The two extremes could be permuted to give four different forms: shy *and* boastful behaviour may both be accompanied by feelings of inferiority; or outward shyness *and* outward boastfulness may go with an inner attitude of superiority.

Form (1), shy behaviour combined with feelings of inferiority, is commonly called shyness.

Form (2), boastful behaviour which serves as a screen for feelings of shyness.

Form (3), shy behaviour combined with feelings of real or inflated superiority; to this form belong those people who suddenly cease to be shy when they have an occasion to shine and who stay more or less involuntarily in the background. These persons may not be conscious of their desire to be in the centre of attention.

Form (4) shows relatively uninhibited self-display which is outside my subject; it is, however, problematical whether this form exists at all.

V. VARIATIONS OF THRESHOLD IN DIFFERENT NATIONS

Although constitutional differences exist, social influence and education are important in producing shyness. We might say that shyness is almost an effect of education. American surveys (6) have shown that teachers do not consider shyness in school children as a problem, because this type of behaviour does not give them any trouble, as do, for example, open rebellion, lying or stealing. The teachers' attitudes therefore set a standard for the child, who knows that by behaving shyly he is more apt to win praise than punishment.

As shyness is a social phenomenon, it is not astonishing that its threshold varies according to the standards and ideals of the group. This variation can easily be proved by a reference to language, for clearly different nations have different notions about this form of behaviour. The French, Spanish, Italian and Latin languages do not possess a single word—like shy—which covers the whole complex. In all these languages the notion is decomposed into three main factors: fear, shame and hostility. There is only one word in French, *se gêner*, which corresponds closely to the English expression 'to be shy'. The word *gêne* is derived from Hebrew *gehenna* denoting 'hell, a place of torture' (7). This etymology shows clearly how the French temperament judges the inhibitions connected with shyness and social conventions.

Apart from the words *scheu* and *schüchtern*, there is the German word *blöde* which meant originally 'weak, delicate, shy'. During the last 50 years the meaning of this word has changed slowly and *blöde* denotes now only stupidity; this contemptuous judgement has at the same time influenced the judgement of shyness which is considered in Germany as a form of slight stupidity.

The English language is by far the richest in expressions for shyness, and the English seem to be relatively most sympathetic to it. In a nation whose 'understatement' is a proverbial characteristic, this strong sympathy for shyness is not astonishing. The 'understatement' seems to represent form (3), where inner feelings of superiority are coupled with outward shyness.

The list of synonyms for shyness in different languages shows a preponderance of fear and mistrust and a smaller proportion of shame and embarrassment. It is interesting to note that the feelings of inferiority which play such a great part in the individual's consciousness are not specially expressed in the different languages. There is only the English word 'diffident', which forms a connecting link between mistrust and the feeling of inferiority.

The change from passivity to activity in boastfulness is very clearly expressed by language: instead of the individual himself being shocked, he behaves in a shocking way, instead of being embarrassed, he embarrasses others.

VI. UNCONSCIOUS BACKGROUND OF SHYNESS

As the shy individual is not curtailing his actions voluntarily, we have to assume that unconscious factors are at work in producing the inhibition which seems to have been partially adapted to reality during the individual's childhood. As the lack of inhibition in boastfulness does not allow the making of friendly contacts either, we can safely assume that the instincts, emotions and actions inhibited in shyness are asocial. What does the fear in shyness inhibit? Comparing shyness with boastfulness, it appears that in the shy person aggression is inhibited. A further hint in the same direction is the well-known irascibility of shy people when sudden fits of anger and bad temper break through their usual mildness (8).

A number of synonyms for shyness are derived from words which denote hostility and ferocity. There is further the so-called *esprit de l'escalier*[1] where biting remarks come into the individual's mind when the occasion for uttering them has passed. Clumsiness (13) which is often observed in shy people—regarded as a blunder—may also represent an outbreak of repressed aggression.

Shyness and boastfulness are both at work in the attack arising from fear (*Angstangriff* (9)). Here fear changes into aggression. This fear may be either fear of real events or fear of the strength of one's own instincts, one's own aggression, just as a prison wall is both a defence against people coming in from outside and against escape from within. We may venture to say that the fear in shyness inhibits aggression, and that in boisterousness the fear has possibly increased to such an extent as to necessitate an outbreak of aggression in self-defence.

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[1] This expression was coined by J. J. Rousseau in his *Confessions*.

If we consider the inhibiting factor of shame, we find a very close connexion between shame and sexuality; modesty and self-display. Before the individual shows signs of shame in connexion with moral laws, he is ashamed of his own body, its functions and those actions, thoughts, and words which refer to it. The compromise between shyness and self-display is clearly seen in the symptom of blushing. The blushing person unwittingly attracts more attention, so that a repressed desire for self-display and exhibitionism is gratified in this symptom (10). Comparatively uninhibited self-display comes out very often in boisterousness. Language gives us here an amusing support. The expression 'to strut' which is sometimes used to describe the pompousness of a person shows in its etymology a possible connexion with genital exhibitionism. The word in its original sense means 'to stick out stiffly' (11). The connexion between shame and sexuality is expressed clearly by the English, German, French, Latin and Spanish languages; e.g. till to-day the word *Scham* denotes in German the feeling of shame and the external masculine or feminine genital parts. In English the double meaning of the word 'shame' ceased about the beginning of the seventeenth century.

Connected with both fear and shame is secrecy. If we link it with the original objects of shame, the body, actions and thoughts concerning it, the possible connexion with masturbation suggests itself, all the more when we take into consideration the possible existence of guilt feelings (which may also be expressed through blushing), the pre-occupation with the self and the infantilism of the shy individual. Some of the stereotyped movements and symptomatic actions usually connected with deficient mastery of limbs belong to this realm, e.g. fidgety hand movements of shy people. The connexion of fear and shame with masturbation seems to be due to early educational prohibitions.

While the shy individual is usually aware of his feelings of fear and shame—although he does not know against what these inhibiting barriers are directed—he will usually not admit feeling any mistrust of people. This is not at all astonishing as fear and shame are socially accepted emotions or propensities, while mistrust is socially disapproved. The individual's constant effort to repress the reappearing unconscious material of aggression and sexuality needs the mistrust as a guardian against outside attempts to penetrate or loosen the barrier. The fact that the shy person's instincts have to be under constant repression and control is the reason why little deflexion or sublimation is possible; but as his instincts are neither deflected nor sublimated they have to be kept under repression and control. This is the vicious circle in which the ego of the shy individual is imprisoned.

VII. Character and personality

With regard to character and personality the shy individual belongs to the group of narcissistic[1] characters with mainly negative and egocentric tendencies, although he may behave in an apparently altruistic manner. The constant guarding and watching leads to the formation of a rigid character which is not flexible enough to embrace new experiences or to adapt itself to new situations and requirements.

The social and emotional immaturity of the shy individual points to a damage in development. There are doubtless possible connexions with different developmental phases of childhood, specially with the questions of masochism, sadism and castration fear. But there is up till now too little evidence to allow more than mention of this. The shy individual seems to behave like an impotent child, longing to be admitted to the group of potent adults (fathers), being at the same time afraid of being too inferior and fearing to be snubbed for his presumptuous wishes. This fear of being snubbed represents in a figurative sense castration fear. Etymology shows also a literal connexion. The verb 'to snub' meant originally 'to snip off the end of a thing'. The Icelandic *snubbotr* means 'snubbed, nipped, with the tip cut off'. 'To snub' is allied to the Swedish *snöpa* which means 'to castrate' (11).

VIII. Neurosis and psychosis

The combination of subjective mental suffering, inability to change this state of mind voluntarily and impaired relations with the environment, is characteristic of psychoneurosis. Therefore we have to consider shyness as a psychoneurotic syndrome. A clear diagnosis of the type of neurosis is not possible. Shyness has in common with conversion hysteria the symptomatology, ambivalence, instability and sexual components; with anxiety neurosis, fear and inhibition; while indecision, mistrust and aggressive components belong to the symptomatology of obsessional neurosis. The constant watching of the mental state of the shy person seems to run parallel to the constant watching of the organic side of the hypochondriac person. This watching from without—known as depersonalization when reaching a high grade—is found in schizophrenia and also in melancholia. A state of severe shyness may recall schizophrenic attitudes to such a degree as to cause diagnostic difficulties. Many investigators have emphasized the similarities between simple dementia praecox and shyness (1,2,12), but there are also features common to paranoia, e.g. increased references to the self and the exaggerated preparedness to associate and relate outer happenings to themselves.

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[1] Etymology lays special stress on the point that narcissism designates not only the direction towards the self, but at the same time the exclusion of the outer world. *Narcissus* and 'narcosis' have the common Greek root: ναρκόω = I benumb; ναρκάω = I grow numb (11).

The change between shyness and boastfulness can be clearly seen in drug addicts; it is characteristic of, for example, alcoholism or morphinism that the individual—usually inhibited and withdrawn—is cheerful and boastful under the influence of the drug.

IX. SUMMARY

Summarizing, it may be said that shyness is a state of hyper-inhibition through fear, shame and mistrust, directed partly against the environment, partly against the subject's own impulses, mainly aggression and sexuality.[1]

Shyness is a psycho-pathological state which shows connexion with psychoneuroses and with psychoses, especially of the schizophrenic group.

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[1] These findings may help towards the understanding e.g. of functional enuresis. A high percentage of enuretic children is shy (about 85 % at the Manchester Child Guidance Clinic); the overcoming of their shyness, that is, the acceptance, recognition and expression (and possibly later the suppression) of their own impulses, specially their aggression, has been shown to have a beneficial influence on their enuresis.

AN EXAMINATION OF THE VALUE OF THE EMPIRICAL APPROACH TO AESTHETICS

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I. *Introduction* (pp. 114–116).
II. *Assumptions implied in an empirical aesthetics* (pp. 116–120).
III. *Classification of the experimental categories* (pp. 120–122).
IV. *Types of hypothesis related to each class, and their value* (pp. 122–128).
V. *Conclusion* (pp. 128–129).
References (p. 130).

I. INTRODUCTION

In view of the growing tendency characteristic of modern psychology to regard the study of human behaviour as an exact science, to question the value of any of the vast and ever-growing accumulation of statistical data derived from the mathematical analysis of controlled human behaviour is to invite the criticism that any such doubts are but the product of a die-hard and reactionary conservatism. Whatever may be the value of the statistical approach to general psychology, however, the application of the empirical method to the study of aesthetics has a value sufficiently restricted to justify an examination of its usefulness and of its limitations, at least in this field. A further need for such investigation lies in the intrusion into the literature of aesthetics of views on the value of the method ranging from an irritated contempt for any other form of investigation to the denial of the possibility of an 'exact aesthetics'. Prof. Seashore, in introducing the fourth volume of the *Iowa Studies in the Psychology of Music*, expressed the hope that "the jargon of arm-chair theories will be replaced gradually by an adequate scientific aesthetics" ((1), p. 5). On the other hand, Lord Listowel asks: "Can the surest and most modern of scientific methods, by means of which the conditions of an event are deliberately varied and controlled, a method admirably suited to the investigation of the material world by chemists and physicists, and gaining ground rapidly in the realm of living organisms, be employed with equal success to elucidate so fragile and evanescent a flower of the mind as beauty?" ((2), p. 165).

The most obvious argument in justification of the method is that the empirically and mathematically educed data provide material from which the aesthetician may construct, or by which he may test the accuracy of, his hypotheses, in the certainty that at least—however nebulous may be his ultimate conclusions—he has something definite on which to build. Unfortunately, he is not always convinced that the carefully elaborated mathematical superstructure is itself based on anything so exact, definite and certain. The class of experiment which is based on relative aesthetic preferences, for instance, examples of which are scattered throughout the voluminous records of empirical aesthetics, is open to suspicion and often in fact suggests more problems than it solves. One such problem is the nature and assumed constancy of the judgement of preference, the unit on which the whole investigation depends. Perhaps it may be permitted to take as a typical example the recently published researches of Eysenck (3).

In one of these, fifteen subjects of ages varying from twenty years to seventy, including artists, university students, bank clerks, typists and teachers, were asked to rank in "order of liking" a number of postcard reproductions of landscape paintings and portraits and a number of photographs of statues and of landscapes, selected "with a view to making them equal in 'goodness'". One immediately wonders on what individual grounds the preferences were made and by what factors of individual experience the choice was influenced. What is the nature, the qualitative content, of the judgements which form the ultimate unit of the mathematical analysis? Can it be assumed to be qualitatively constant, a unit, when the fifteen subjects, carefully selected to be a group representative of the general public, are for that reason so varied in age and in aesthetic experience? In the case of the artists the preferences may reasonably be assumed to be genuine aesthetic judgements and, therefore, related to the 'goodness' of the pictures. But what is this 'goodness'? Is there any relation between aesthetic value and degree of preference as measured by the sum of preferences? It is a matter of common observation that there is no such relation. There are no queues for the art galleries; it is not the symphony which pleases what Dr Bridges termed 'the common ear'; the lyrical gem which delights the lover of poetry has no charm for the multitude. If the degree of popular preference, therefore, has no relation to aesthetic value, what is its value to the study of aesthetics? Moreover, if some of the subjects are expressing a preference which is related to aesthetic value, while others are expressing preferences influenced probably by associations—as Valentine has shown them often to be (4)—or by other personal tendencies and aesthetically irrelevant factors such as youth, extraversion, or radicalism, as Eysenck has suggested, the unit on which the mathematical analysis is based has no qualitative constancy. The main conclusion, that the more youthful, radical and objectively minded members of society tend in general to prefer the brighter things, including brighter pictures, may be of general psychological interest; but this can hardly be regarded as 'a factor in aesthetic judgements', for it bears no relation to aesthetic value. If all the pictures of the world were classified as bright and dull, few would deny that each category would include every degree of aesthetic value from an immortal beauty to the banal, trivial and counterfeit.

It is clear at least that the value of empirical aesthetics is not self-evident, and that the extent of its usefulness should be investigated. In this paper it has been possible only to examine some of the assumptions on which the possibility of an empirical aesthetics is based, to attempt a classification of the kinds of experiment which can be employed, and to examine the kinds of hypothesis which can be formulated in each of the experimental classes.

II. ASSUMPTIONS IMPLIED IN AN EMPIRICAL AESTHETICS

Aesthetic experience may be defined as that experience which tends to evoke an aesthetic judgement; that is, a proposition that something is, or is not, beautiful. Notwithstanding the extent of individual differences in the perception and judgement of beauty, and notwithstanding the infinite diversity of things so judged, it is clear that the judgement of beauty is a generally intelligible concept. However much one may question a specific judgement of beauty, the meaning of such a judgement is universally understood. It is irrational, moreover, not to assume that this meaning has a qualitative constancy, for the same objects (again notwithstanding their infinite diversity) tend and have always tended to evoke in general the same judgement. We can still find beauty in the cave drawings of Palaeolithic Man, in the surviving poetry of the ancients, even in translation, in the sculpture of the Greeks, and share their delight in them, as we can presume their joy in the sunset and flowers, in colour and form, as we can understand their love of music. It is only by postulating the constant and recognizable quality of the affective state or subjective experience evoked by the objects judged beautiful that any meaning can be assigned to the aesthetic judgement, for it is the only factor discoverable common to all aesthetic experience. While it is fallacious to argue, as believers in 'significant form' would appear to argue, that all objects which evoke a particular kind of subjective reaction must, therefore, have some other common character, that is, other than their common ability to evoke the response, it is not fallacious to assume that they may have. Empirical aesthetics makes this assumption, and as the objective factor in the aesthetic situation is capable of scientific control, it is possible both to investigate objective data generally recognized as beautiful for the purpose of objective analysis, and to control stimuli selected for investigation in relation to selected and non-selected 'experiencers' or 'subjects'.

Fechner is generally regarded as the founder of exact aesthetics, in that he first systematically propounded as a method the advantages of approaching the problem 'von unten', empirically, instead of 'descending' metaphysically from general concepts, 'von oben', to the particular. Fechner differs, however, from many of his modern disciples in that he did not denounce the metaphysical approach as valueless, but held the view that "a philosophy of aesthetic can be constructed after and over an empirical aesthetic just as a philosophy of nature can be erected on physiology and physics" ((2), p. 40). It is an exaggeration of the scientific exactitude of the empirical method, however, to claim, as has been claimed, that it makes no a priori assumptions ((2), pp. 9–10).

That there are in fact many such assumptions, presumably ignored by the empiricists as self-evident truths, is obvious, for otherwise the experiments themselves have no co-ordinating factor; that these assumptions are not self-evident truths is demonstrable. One such assumption which has prevailed since Fechner's deliberate employment of it is that relative aesthetic values of certain aesthetic stimuli are determinable by the degree of pleasure evoked by them. That this is an assumption the truth of which is not self-evident and is in fact doubtful has already been demonstrated by reference to the observable fact that most people find their greatest pleasure, in this department of human experience, in the spurious and gaudy, the crude and tawdry, the sentimental and imitative, the rhythmic jingle or vulgar ornament. Nor is the assumption made more acceptable when the pleasure is qualified as 'genuine aesthetic pleasure', for the only difference between the enjoyment of a bad work of art and that of a good one is in the work itself—and even critics disagree. The relevant point is that at least one of the assumptions which are fundamental to a large proportion of the experiments of the empirical aestheticians is not a self-evident truth.

Another assumption, though questioned by such careful investigators as Valentine, is that the analysis of the aesthetic response to simple stimuli can be utilized to throw light on those complex forms which are constructed from such simple elements. "Of course we shall find great changes in the effects of colours or lines when they are built up into complex arrangements such as pictures. But we may reasonably expect some of their most striking and characteristic effects to remain even then" ((4), p. 11). While this assumption, thus qualified by Valentine, is almost certainly true, that it is not self-evident or necessarily true is proved by the existence of opposed views. Richards, who denies the truth of the assumption, holds the view that "the more simple the object contemplated, the more varied the responses will be which can be expected from it" ((5), p. 9).[1]

A more important and elusive though allied assumption is that the investigated characteristic is the actual stimulus of the aesthetic response. In such an experiment as, for example, one which involves the use of Zeisung's 'golden section', where a relation is objectively isolated, the assumption appears to be self-evident, though the element of association in stimuli apparently so isolated as single colours makes even such an assumption as this capable of doubt. A different kind of experiment better illustrates the doubtful nature of the assumption. An extraordinary amount of labour has been directed towards the analysis of minute variations from mechanical accuracy in the interpretation of music by solo performers of recognized efficiency and 'artistry' under controlled conditions. The perfection of the methods of phonography has enabled the most minute variations in pitch, time, and intensity of sounds to be accurately recorded and to be graphically represented for investigation(6, 7, 8). The question arises, however, as to whether the beauty of the work or that of the performance of it is being investigated. Volume IV of the *Iowa Studies in the Psychology of Music* includes a complete diagrammatic record of the First Movement of Beethoven's '*Moonlight*' *Sonata*, as played by Prof. Philip Greeley Clapp. From the record every slight variation in time and intensity is apparent, and it is possible even to see how long the thumb is held down in the arpeggio passages. But does the beauty of the Beethoven fragment depend on these departures from the precise relations indicated in the score? It is obvious that it neither does nor can do. The same quality of interpretation can be applied to a work of any intrinsic quality, and any aesthetic value which the interpretation itself may have is distinct from the aesthetic interest which the work itself may arouse. Moreover, unless there is some common factor discoverable in the variations themselves, as there are common tendencies in the violin and vocal vibrato, such experiments serve no possible purpose except to expose the nature and extent of personal idiosyncrasy. That there is any but a negligible degree of consistency even in the repeated performances of the same work by the same artist has been demonstrated by the experimentalists themselves ((1), pp. 342 sqq.). The mere fact that variation from the regular is a factor which influences aesthetic experience by its tendency to maintain and restimulate aesthetic interest is demonstrable without so elaborate an investigation. One does not need to measure the variations in the rose petal to know that roses would be of less permanent aesthetic interest if, in Robert Bridges' phrase, they were "as similar in shape as all equilateral triangles" ((9), p. 289).

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[1] Prof. Valentine's comment on this view is "Even so (if true), the more likely are they to include all the possible responses to a more complex object, and so to help us to look out for these".

One of the most valuable of the assumptions accepted as a basis for experimentation is that the developed judgement of taste is a cultured derivative from innate aesthetic tendencies in the child and, in a modified form, in primitive man. In the investigation of such innate tendencies and of the kind of influences which in general modify their development, experimental aesthetics has made a valuable contribution to the study of the aesthetic experience.

There are two final assumptions, important because they are tacitly assumed by experimenter and subject alike, and accepted by child and adult, artist and psychologist, with unquestioning certainty. One is that the experience is always the same kind of experience, irrespective of the nature of the stimulus. This may be anything from a coloured surface, a rectangle, a musical interval or a group of rhythmic nonsense syllables to an incomparable work of art in any medium; the investigation may be an analysis of the colour preferences of an infant or of the degree of *portamente* a singer employs in passing from one note to another; in all these and all such investigations it is always assumed as obvious that they are all directed towards the same ultimate purpose and, therefore, that the aesthetic experience is qualitatively constant.

The second of these tacit assumptions is that the phenomenon which is ultimately being tracked down is that of beauty. Prof. Valentine's book on empirical aesthetics is, for example, entitled *An Introduction to the Experimental Psychology of Beauty*. The general acceptance of these assumptions is shown by the fact that no reference is ever made to either of them. There is no question about the *fact* of beauty, and there is no mystery or doubt, in the general mind, about the *fact* of its equal manifestation in situations of an infinite diversity.

III. CLASSIFICATION OF THE EXPERIMENTAL CATEGORIES

In any aesthetic situation there can be only three elements: an object, regarded as constituting the aesthetic stimulus; a subject, stimulated into some kind of reaction; and the relation between them. As the investigator can direct his experiment towards any one of these three elements, there are, therefore, three fundamental classes of experimental aesthetics. Külpe's classification differentiates the experiments in respect to the relation between subject and object; if this scheme is used as a basis, and the objective and subjective classes are superimposed on this classification, it is possible thereby to obtain a complete analysis of all the possible kinds of experiment, because such a classification exhausts all possible elements in the situation.

Külpe (10) exhausts the classification of the possible relations between subject and object by dividing the experiments into three categories. His first class is that in which the relation is that of impression (*Eindrucksmethode*); his second is that in which the relation is that of expression (*Ausdrucksmethode*); and his third is that in which the relation is productive or manipulative (*Herstellung*).

In the first class the experiment is directed towards the relative valuation of controlled stimuli in accordance with the degree of aesthetic satisfaction such stimuli appear to be capable of evoking. The second class is that especially used by Valentine, and is based on the subjective analysis of mental states which a specific experience tends to evoke in different subjects. From this class, for example, are derived all the experimentally produced data from which are deduced the hypotheses which formulate such influences as that exerted by association on the aesthetic experience. The third class provides material which the subject himself can control and manipulate for the production of an aesthetic end. The necessity of eliminating skill and specific knowledge, not only because they are essentially irrelevant factors but more especially because their presence disturbs the control of the experiment by destroying subject equality, restricts the utility of this class of investigation. It is, however, a valuable kind of experiment and one capable of considerable development, if sufficient caution is exercised in its control and in the analysis of the evidence furnished.

It is obviously possible to extend this classification by grouping the different kinds of 'stimuli' employed and by grouping the different kinds of subjects employed. There are four kinds of 'objects' available for such experimentation, and of these the fourth is a specific variety of the third. It is possible to use, first, a simple sensory stimulus, such as a colour, a line, a sound, a scent, or a surface intended to be touched. It is possible, secondly, to use two or more of such elements in controlled relation, as in experiments with musical intervals or linear relations. A point, subsequently developed, is that this class of experiments assumes that a *relation* between two simple sensory elements or 'fundaments' may have aesthetic value, as the former class assumes that the simple apprehension of relatively isolated sentient experience may reveal such value in the fundaments themselves. An isolated sound, like the note of a flute, may be judged beautiful, and a relation between two sounds, a musical interval or 'chord' may be so judged and preferred to another interval in musical relation, though the aesthetic value of the sounds in isolation remain constant. Thirdly it is possible to use works of art as objective 'stimuli', and, lastly, reproductions of natural phenomena, such as photographs and sound records. In these two classes the relations are, respectively, aesthetically purposed by the artist and achieved by Nature, and in neither case subject to experimental control other than that of selection.

Logically this exhausts the possible objective variants, but the transient nature of sound as opposed to the relative permanence of visual art introduces a confusing factor into the psychological study of the art of music. Except where there exists the rare ability to extract any aesthetic satisfaction from a music score, and assuming, as is obviously true, that music is intended to be heard, music differs from the visual arts in that its presentation to the 'subject', the listener or the contemplator, depends on an interpreter who is not necessarily and is but rarely the creator of the work. Thus, while the sculptor and the painter leave for posterity their own work for direct and immediate contemplation, the musician leaves but a record of symbols for future executants to reconvert into the sounds from which its aesthetic value has to be judged. Thus, a fifth class of objective data may be used, the records of 'performed' musical works, differing from the products of visual art in that the interpretative modifications of the performer are superimposed factors in the valuation of the work.

The true relation between the intrinsic value of a musical composition and its interpretation by a soloist or by an orchestra, or some other group of executant musicians, is clear if the same relation is sought in the world of the poets. Poetry, like music, may be recorded symbolically, but it differs in that the ability to read literary symbols silently, and to appreciate the value of what is read, is much more general than is the ability to read silently, and to appreciate by that process, a music score. But it would be absurd to pretend that the beauty of a poem depended on the inflection of the voice in reading or on any other quality incidental to its actual interpretation by a reader, or, as the Iowa School of empirical psychologists claim for a music score, that the written work is 'only a schematic reference' about which the interpreter weaves his own imaginative interpretation.

It is possible to use the third and fourth classes of objective material either in relation to a subject or group of subjects, as the first and second classes must be used, or, their intrinsic beauty having been assumed, they may be objectively analysed for possible common and constant characteristics.

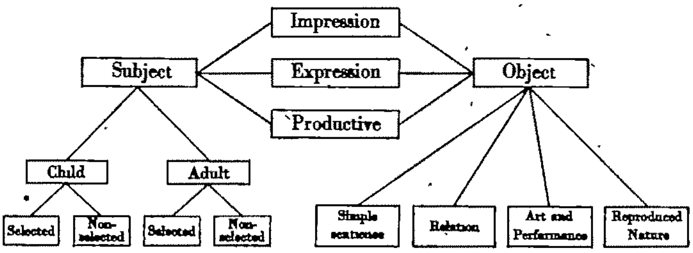

Fig. 1.

The final classification is that which is based on the selection or control of the subject. Here again it is easy to exhaust the possible categories. The most valuable division is that of children and adults, and either may be 'non-selected', and therefore, if sufficiently numerous, presumably normal or typical of general childhood or of the adult public; or they may be selected so that the group may represent a specific class. The subjects may, for example, be of a given age, a given sex, or environment, or standard of intelligence; the adults may be selected artists, performers, critics, undergraduates, trained psychologists, or what not.

It may be permissible to represent this triple classification graphically, as in Fig. 1.

IV. TYPES OF HYPOTHESIS RELATED TO EACH CLASS, AND THEIR VALUE

It is neither possible in a paper of this scope, nor relevant to its purpose, to refer to more of the actual experiments than those which illustrate this classification or which furnish data as to the kind of hypothesis which is derivable from each of the classes. Typical of the Impression method, the method of differentiating controlled stimuli in relation to their relative aesthetic value as judged by their tendency to impress the subject, are the experiments of Féré(11), Winch(12), Valentine(13), Bullough(14,15) and Martin(16), who have investigated the preferences of children and adults in simple sensory stimuli such as colour and line. Similar preferences for specifically related stimuli in such concepts as rhythm, symmetry, balance, musical intervals and linear relations have been investigated by Myers(17), Valentine(18), Ethel Puffer(19), Tennant(20) and Lowery(21). The Expression method takes different forms, but the most obviously typical form is that in which the subject is asked to express a reason for a stated preference, as in Prof. Valentine's association experiments. Other interesting experiments, typical of this class, have been made by Tennant(20) on colour contrast, by Bugg(22) on consonance judgement, by Cicely Parsons(23) on children's interpretations of ink-blots, and by Feasey(24) on children's appreciation of poetry.

What kinds of hypotheses have been or can be made from the data discovered from such investigations? And what is the relation of such hypotheses to the fundamental problems of aesthetics?

The first kind of hypothesis is that which relates to the problem of general aesthetic value, and a valuable hypothesis of this class is that the possibility of aesthetic preference tends to be accepted as a fact without question. Irrespective of the nature of the objective stimuli employed, irrespective of the age or type of subject, whether child or adult, the possibility of evaluating such stimuli aesthetically in accordance with the degree of pleasure experienced in response to them; is a possibility which is invariably understood, and the question is regarded as a perfectly natural one. The very fact that this truth has, so far as I know, escaped special comment is significant, as further evidence of the general tendency to accept the assumption as so self-evident a truth as to be unworthy of comment. But, as has been previously suggested, the implied assumption is not a self-evident truth; nor is the hypothesis a negligible one. If the investigation were restricted to the first class of experiments, the hypothesis could be stated in the form of the proposition that *A* is intrinsically more beautiful or less beautiful than *B* (for it has already been assumed that the phenomenon investigated is that of beauty, and that the evoked experience is qualitatively constant). The reasonable consistency shown in the order of the 'aesthetic preference' of such stimuli as musical intervals or colours suggests the truth of this proposition. But it is not the whole truth. It ignores the possibility that such quantitative differentiation may be determined, or at least influenced, by subjective factors.

The second class of investigation furnishes evidence of the existence of many such factors, which tend to influence and to modify the aesthetic judgement. The most important work of this class of investigation is the establishment of general principles of quantitative aesthetic interference by subjective tendencies or influences. Many examples of such generalized hypotheses may be found in the records of relevant experiments. Prof. Valentine writes, for example, that "Associations may occur with all persons at all stages of aesthetic culture, but a frequent use of them as the main basis of the appreciation of a picture...seems to be indicative of a low degree of aesthetic development, at least in this department of art" ((4), p. 86). "If the aesthetic experience is to be enjoyed to the full, there must be no rude interruption in the course of the mental apprehension of the object" ((4), p. 97). "Constant repetition generally produces boredom and disgust, except, perhaps, in the case of the very finest work of art" ((4), p. 112). This is an interesting hypothesis when brought into relation with the apparent paradox "Familiarity is an essential element of the idea of beauty" ((4), p. 111).

The hypothesis that the judgement of aesthetic value is modifiable by subjective, psychological influences suggests that no objective character can, alone, be accepted as evidence of beauty. As Kant expressed it, propositions of the form "All objects which have such and such qualities are beautiful" have no universal validity. Beauty can neither be created nor judged by reference to an objective formula; but any established hypothesis which generalizes the modifying tendency of some psychological or subjective influence is valuable knowledge to the aesthetician, the artist and the critic. Such a proposition as "No objects which have such and such qualities are beautiful" could clearly have universal validity, for though awareness of beauty can never depend on rational judgement, reasons as to why an experience is not beautiful may be deducible, and would be capable of general formulation in the terms of the principles which the second class of experimental investigation seeks to establish.

The kind of hypothesis which can be evolved from the purely objective analysis of works of art has all the dangers of the first class of investigation, that of the relative aesthetic value of general objective stimuli, with the additional dangers due to the fact that art is but a department of aesthetics. The one quality common to all works of art is that they are designed, in the sense that they are deliberately purposed and controlled.. Design, therefore, may be regarded as a constant quality of art, even when the element of form is least apparent, as in a landscape painting or a portrait. A genuine work of pictorial art, even though it achieve an apparent realism, is a deliberate composition, constructed as a synthetic unity from selected and modified elements from the artist's experience, and can never be a merely mechanical and imitative reproduction of Nature. Music is a structure of formally related sounds, and a musical composition must achieve formal unity. Both these facts are true of poetry too and, when translated into their respective media, are true of all arts. From Aristotle's 'unity in variety' to Clive Bell's 'significant form', this quality has been regarded as a characteristic, or as the characteristic, of objects regarded as beautiful, or at least of works of art, for it is difficult to find 'significant form' in Nature. But any attempt to postulate any doctrine based on formal characteristics, or to define beauty or even a specific art in terms of form, always fails and must always fail. They must fail because there is no such valid proposition as "Objects which possess such and such qualities are beautiful". To judge a work of art as beautiful, or successful, because it is consistent with some formal principle, is to make a rational and not an aesthetic judgement. Moreover, it is all too easily demonstrable that many works which have formal unity are not beautiful, and that relative aesthetic value is not proportionate to the degree of formal significance in art. There is a great deal more formalism in a patterned wall-paper than in a mezzotint by Frank Short, but a miraculously conceived formalism could never achieve an equal beauty.

To say that the beauty of a work of art does not depend merely on its formal structure is not to say that its beauty is not influenced by its formal structure. Lack of unity may destroy beauty, though alone it is not sufficient to create it.

Paintings, poetry and music have been used in the second class of experiments, that is, for the investigation of the influences affecting the evoked experience of children and adults representative of many different environments. A most interesting and illuminating comment, incidental to Margaret Bulley's experiments with pairs of pictures, similar in subject but of contrasted aesthetic value, was made by a boy 'subject'. He said, "It is quite easy to choose. All that I think ugly I say are art, and all that I think beautiful I say are not art" ((25), Introd. p. viii).

This frank confession summarizes much of the truth which the mass of evidence furnished by Miss Bulley's and similar experiments tends to confirm. The words "All that I think ugly" and "All that I think beautiful" perfectly express the spontaneous, individual and 'noegenetic' quality of the aesthetic judgement and the existence of what has often been called the 'intuition of beauty'. The comment also expresses, however, the great divergence between this innate ability which has created the universally intelligible concept of beauty, and the developed appreciative judgement of art. It is this modifiability of the aesthetic judgement which explains the impossibility of assessing aesthetic value of any but the simplest kinds of stimuli in terms of a generally evoked pleasure.

That the appreciation of art demands both a general cultural development and specific knowledge and experience is evidenced by the extraordinary irrelevance which characterizes the influences confessed as reasons for the judgements. Reminiscent experiences and associations of a purely personal nature, however much they may be allowed to intrude into an individual contemplation of a work of art, are obviously unrelated to the beauty of the work or, to express it differently, to its general aesthetic value. Yet the results show that in the majority of cases the judgement is influenced by personal and irrelevant associations and prejudicial dispositions ((25), Appendix I and Supplement).

This may, to some extent, be due to the nature of the experiments, which sometimes ask for a reason or, when no reason is demanded, suggest that it may be offered. Valentine has observed the difficulty experienced by many of his subjects in assigning a reason for their approved choice, and the relative ease with which it is possible to explain disapproval ((4), p. 70). This is true, of course, because a genuine aesthetic judgement is never a rational judgement but, as has already been shown, the truth of this does not deny the possibility of criticism.

More encouraging is the evidence, and it is considerable, that genuine aesthetic judgements do occur even with very young children. The child who writes "There are lovely curves in it", or a boy of six who writes "The sky is nice", is making a genuine, unspoiled aesthetic judgement, very different from a preference made "because I like to play cowboys" ((25), pp. 86 and 87).

The modifiability of the aesthetic judgement either as the result of purposive training or through the accidental influences of environment does not imply that a developed judgement (or a spoiled judgement) is consciously influenced by relevant or irrelevant knowledge, but that the tendency to find aesthetic satisfaction in specific situations is itself capable of change.

Finally, this class of investigation has shown that this modifiability is, in part, a natural development like growth. Evidence for this is furnished by the consistency of the 'age-appreciation' curves for children drawn from widely differing environments, evidence made more conclusive by the curious nature of the curve itself ((25), pp. 88 and 92). The group testing of thousands of children, from England, France, Cyprus and Japan, showed a consistent tendency for the appreciation curve to *fall* from about the age of six years to that of nine or ten years, and subsequently to rise. In one group only, where specific training had been given, the subsequent rise was consistent and rapid to the age of fifteen years, but in this case, too, the same preliminary drop occurred. It has been suggested that this "decline in taste that is so marked as the child grows older is probably due to the fact that the child is then outgrowing its life of fantasy and imagination, and is entering into a phase of extreme realism when it is powerfully attracted by everything practical" ((25) p. 85). It is inevitable that the use of things and their causes and effects should tend, in varying degrees but increasingly, to divert the child's interest from their 'attractiveness' and proportionately to replace an interest which has no utilitarian or rational significance. Unfortunately the original aesthetic tendency is not always fully revived.

The third class of experiments, the productive or manipulative class, presents a fundamental difficulty of procedure. Unless the materials supplied for manipulation are so simple as to restrict their capacity for aesthetic readjustment below the limit of usefulness, the influence of skill and experience enters to destroy the control of the experiment. A further difficulty is how to use the resultant products. How and by what criteria are they to be relatively evaluated? What evidence could they furnish sufficiently reliable for the deduction of any general hypotheses?

An interesting experiment which went far towards a solution of the first problem was made by C. V. Mackenzie, who used as a medium the soft 'rubber' bricks employed in certain building operations. Each member of a boys' art class was given a brick and a number of simple tools, and told that there was a head inside the brick if he were capable of finding it. The results, which I saw, were of an astonishing variety, displaying deliberately purposed conceptions and considerable ingenuity in the adaptation of accidental results (26). The task showed that many of the actual difficulties characteristic of the actual craftsmanship involved in the art of sculpture had revealed themselves, a fact which must have influenced the boys' subsequent attitude to sculpture, where these difficulties had been solved. While such experiments as this play an important part in the training of the craftsmanship of an art and incidentally influence the appreciative attitude to an art, they afford little evidence of value to the aesthetician. An artist must be a craftsman but a craftsman need not be an artist. The relevant skills have to be acquired and the nature of the medium has to be mastered before the process can be made deliberately subservient to a specific end; and, when this degree of efficiency has been acquired, the end need not be an aesthetic one, nor, when aesthetic, need it be successfully accomplished. In the stage of experimentation the interest inevitably is directed towards the manipulative process and activity, even, to a large extent, when the process is one of simple patterning.

The difficulties and limitations of the manipulative and productive experiments are still greater in music, though the processes are equally valuable as training. Even in the most practised medium of all, that of language, the effort to write poetry before the ability to make a rational and adequate expression in prose has been acquired may be useful training, but it can display little beyond the existence of individual differences in ability and aesthetic sensibility. Even a prematurely developed creative ability and craftsmanship in a particular medium does not necessarily postulate the development of an appreciation of art values in other media, which demand for such appreciation a similarly developed experience in the specific art itself or a similar degree of relevant knowledge.

V. CONCLUSION

Empirical aesthetics has, therefore, a great value to the general aesthetician, but a restricted value. It can analyse, by the control of simple stimuli and by the employment of child subjects, the fundamental, undeveloped, genetic principles on which aesthetic development is based; it can establish, as generally true, the influences and tendencies which quantitatively control and modify this development and thereby provide useful data for aesthetic training; by an extension of this method it can supply the critic with norms which might help to explain in certain cases the failure of a specific work to evoke aesthetic satisfaction; when a singer or instrumental soloist has 'performed', it can say exactly what has taken place and provide statistics from which possible consistencies in the interpretation of music could be analysed. But it can supply no single rule whereby the artist can create that which is beautiful, and no single principle by which beauty can be judged.

It is only the exclusive claims which have been made for 'exact aesthetics' which makes the realization of its inevitable limitations necessary. It is difficult to understand why such exclusive claims should be made for this science and for no other. Psychology does not claim that the concept of ethical goodness is the fantastic idea of some armchair philosopher or that the concept of truth is a nebulous hypothesis, yet both these descriptions have been applied to those theories of beauty which have not been evolved from the laboratory. Science is necessarily restricted to the synthetic systemization of observable occurrences or, as in the case of psychology, to observable tendencies. Scientific laws and principles are generalized interrelations of observable events. Science can say what might be expected to occur under certain controlled conditions, because of the observable consistency of interrelated events, but it can offer no reason as to why this should be so. An exact science is subject to the additional restriction of being limited to the use of measurable data, but this, its most exact particular, further illustrates the limitations of science, for all measurement is relative to some arbitrary, comparative, or merely convenient unit, or, as in the case of the measurement of value, to the degree of satisfaction relative to a specific end. As motives, ends and, therefore, values, have no place in a pure science, which ignores the use to which its conclusions are applied, the question arises as to what is measurable, other than in terms of value, in aesthetic experience. Any possible measurement as applied to objective data can be only in units of relative degrees of difference in some common attribute—as in the analysis of fluctuations of pitch in musical performance. Any possible measurement as applied to the subject or subjects can be only in terms of the expressed degree of pleasure evoked by the controlled stimuli. This is hardly 'exact' aesthetics, for it is dependent on a unit that is vague, inconsistent, and subject to the influence of non-measurable quantitative factors. It is, moreover, a unit which is applicable only to elementary simple sentience and which is meaningless when applied to any developed art form, for here recognized aesthetic value is demonstrably unrelated to a measure of popular preference.

Notwithstanding the claims of the formalists and the hedonists, it is almost self-evident that beauty is indefinable in terms of objective characteristics or in terms of pleasure; yet, to be wholly comprehensible within the confines of an exact science, it must necessarily be so defined.

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THE SPEED FACTOR IN PERFORMANCE TESTS

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I. *Introduction* (pp. 131–132).
II. *Cube construction times* (pp. 132–134).
III. *Kohs block designs times* (p. 134).
IV. *Two speed-factors* (p. 135).
References (p. 135).

I. Introduction

In a recent publication of the Scottish Council for Research in Education (2), in discussing the existence and nature of a speed factor among performance tests, I mentioned (on p. 41) certain further enquiries which I then hoped might be carried out by one of my students. The war has prevented this, and I am therefore writing this short note to report some calculations I have myself made,[1] and the tentative conclusions I have come to, regarding the speed factor.

The performance tests were those given to 443 boys and 430 girls by Dr A. M. Macmeeken (1), and were, in addition to a Binet test, the following: (1) Seguin Form Board, (2) Manikin, (3) Stutsman Picture, (4) Red Ridinghood Picture, (5) Healy Picture Completion II, (6) Knox Cube Imitation, (7) Cube Construction, and (8) Kohs Block Designs. The children were all, or practically all, of those born in Scotland on four days in 1926, and they were tested individually when their ages were round about 10½ (ranging from 8:11 to 11:9 in the extreme cases). Age has been eliminated from all the correlations quoted below, by partial correlation. The performance tests were given in the above numbered order, at one sitting (the Binet test later on the same day). For the Seguin Form Board times only were taken, and in this test the child was incited to speed and had three tries to see how quickly he or she could finish. The other tests were scored on the quality of the product, but times were also taken (except in the Knox test), and usually moves were also recorded. Since almost all the children produced perfect scores in Stutsman, and a very large majority did so in Manikin, I used times and not scores for these tests in my analysis. For the other tests (nos. 4–8) I used the scores.

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[1] With considerable assistance from Miss Gullen, Mrs Youd, Miss Mellone, Miss Murray, Mr Nandi, and Mr Lawley.

be '*g*' and 'speed' is indicated. The new variate 10 (Cube Construction times) shows a higher saturation with speed in the girls than in the boys, the difference being of doubtful significance.[1] The boys' diagram might perhaps be taken as in agreement with the hypothesis that an urge to speed given in test 1 had died away in the later tests, but hardly the girls' diagram. However, perhaps the most definite fact is that Cube Construction times (10) are only very slightly saturated with '*g*', and that both the two first centroid factors together account for only a very small portion indeed of the variance of this variate, which is largely independent of both of them.

III. KOHS BLOCK DESIGNS TIMES

To check all this I turned to the last test to be given, Kohs Block Designs, where up to ten different times had to be added to get the total time corresponding to the score, and I had caused this to be done and grids to be made before my attention was drawn to a peculiarity of the times in this case which made them quite unsuitable for my purpose. There were ten sub-tests of increasing difficulty, and it was Dr Macmeeken's custom to stop the test if two successive sub-tests produced no score. The total times therefore were for varying numbers of sub-tests from two to ten, and an artificial correlation is thus produced between a low score and a short time, which by our convention we call a negative correlation.

It may very well be that children who are clever at making the Kohs Block Designs in exact imitation of the pattern are also quicker at it; but this positive correlation between ability and speed, if it exist, is here entirely overwhelmed by the above artificial correlation. The measured correlations between times and scores were (for constant age):

Boys	−0·8124
Girls	−0·8524

The situation was further complicated by the fact that there were time limits, which were often reached, for each sub-test, and the complication was so great that any attempt to arrive at a 'true' time (by, say, adding the time limits for the sub-tests not attempted) had to be abandoned as quite out of the question.

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[1] It is difficult, if not impossible, to test the significance of centroid loadings scientifically since guessed communalities are used. But a comparison of the present diagrams for boys and girls with one another, and with the figure given in (8), p. 39, gives some idea of the accuracy of placing of the dots.

IV. TWO SPEED FACTORS

I now returned to the Cube Construction test data, lest on investigation any difficulty of the same sort should be found there. This test was in three parts, separately timed, and these three times had been added together to give the times used as a variate. As, however, in practically all cases the child had tried all three parts of the test and had been given a score and a time for each, the above difficulty due to breaking off in the middle of the test did not occur. But it is a striking fact that the Cube Construction test data showed no correlation between times and scores, holding thus an intermediate position between the Manikin test on the one hand, where that correlation is positive (+0·3), and the Kohs test on the other, where it is negative (−0·8). It seemed possible therefore that the Cube Construction test was an example where the true positive correlation between ability and speed was exactly balanced by a false negative correlation, produced in some more insidious way than the obvious one observed in Kohs Block Designs, and it is my present opinion that this is indeed the case. There are, it would seem, two influences making for short time, even when the whole test is carried out by every child. The one influence makes one child soon get the correct, or at least a very good, result. The other influence makes another child soon satisfied with any result, even a bad one.

In tests (1) Seguin Form Board, (2) Manikin, and (3) Stutsman the task was easy and the great majority of the children accomplished it perfectly, though at different speeds. But in the Cube Construction test the task was difficult and the performances of the children produced scores spread over a wide range. In those first three 'easy' tests the former of the two influences or factors is at work almost alone. In the difficult test both factors are at work. Some children get short times because they are quick at putting the cubes correctly into position. Others get short times because they are easily and quickly satisfied with a poor shot at the task. If this surmise is correct, it seems to follow that times can only be useful data if they are all times for performing *correctly*, or at least nearly correctly, the task set. Times for performing a task with varying degrees of correctness spread over a wide range seem to be a complex variate of little use in practice.

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DARK ADAPTATION DURING STIMULATION WITH COLOURED LIGHT[1]

By BORIS SEMEONOFF

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II. *Apparatus and technique* (pp. 137–139).
III. *Dark adaptation following preliminary stimulation* (pp. 139–144).
  (1) *The work of Kravkov and Semenovskaja* (pp. 139–141).
  (2) *Experiments on adaptation following stimulation with red light* (pp. 141–143).
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I. INTRODUCTORY

MOST of the work described in this paper was undertaken at the suggestion of Prof. James Drever; the main original purpose was an attempt to find means of enhancing normal powers of dark adaptation. Two lines of approach seemed possible: (1) through preliminary stimulation of one or both eyes with coloured light, following upon some unconfirmed results reported by other investigators and discussed in the present paper; (2) through supplementing the diet with vitamin-rich substances, as described in many studies of abnormal cases; it is hoped to describe this work in a second paper. The original plan was later partly abandoned and partly expanded to cover a number of cognate topics, which are discussed in the present paper or elsewhere.

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[1] Based in part on a paper read before the Scottish Branch of the British Psychological Society, Glasgow, March 1941.

II. APPARATUS AND TECHNIQUE

Perhaps more than any other perceptual situation, dark adaptation must be regarded as susceptible to influence from the apparatus and mode of observation employed. This point will be fully discussed elsewhere, but meanwhile it should be remembered that the form in which results are here presented is largely determined by the experimental conditions, and is not necessarily ideal.

Throughout, dark adaptation has been measured in terms of readings on the adaptometer devised and described by Nagel (8). In essence this consists of a light-source illuminating a milk-glass test-field from behind. Light intensity is controlled in two ways: (1) by the interposition between the source and the field of one or more of three reducing plates each of which cuts down the intensity to one-twentieth of previous value; this arrangement gives four 'levels' or ranges of intensity, corresponding points on which represent light intensities in the ratio 8000 : 400 : 20 : 1. (2) Continuous gradation within each range is obtained by means of a diaphragm, controlled by a handle which can be operated either by the subject or by the experimenter. The instrument is so arranged that the four ranges of intensity overlap to a certain extent. Thus, it is possible, by putting in another plate, further to reduce to one-fifth of its own value an intensity corresponding to the starting-point of the next range of intensities. This is a characteristic of the instrument which may have its effect on results, particularly since one of these overlaps coincides closely with a commonly observed point of inflection of curves of adaptation. The total range of effective intensities given is not less than the ratio $10^6 : 1$. A scale, illuminated from below with red light, allows the experimenter to take readings without in any way interfering with the subject's dark adaptation.

All observations were made in a light-proof room with walls painted dull black. Except on a very few occasions, sessions took place between 2 and 5 p.m., lasted for 2 hr. (or a few minutes more or less), and followed 10–15 min. light adaptation to illumination from a 100 W. blue daylight bulb reflected from a matt white surface. The reflector, which was constructed by Mr W. Fulde, of the Heriot-Watt College, Edinburgh, was made of cardboard supported by wooden struts, and was approximately parabolar in section, the interior dimensions being: height 18 in., width

25 in., depth 14 in. An adjustable wire cradle supported a cardboard lamp-shade containing the 100 W. lamp and pierced with holes to allow for ventilation and reduce temperature. The subject's chin rested on a pad of sponge rubber mounted on a wooden block, in such a way that the reflector entirely covered the field of vision. The illumination obtained varied somewhat at different points on the surface; in the portion normally covered by the subject's gaze it ranged between 35 and 45 f.c. This reflector combines ease in handling and cheapness with a minimum of discomfort for the subject. (In the earlier stages of this work a similar but simpler device of white paper mounted on boards was used.)

Originally it had been intended to study mostly monocular adaptation, but since the subjects reported some degree of discomfort under these conditions binocular observation was substituted. Another change which was considered desirable was to reduce the test field from its original form of a disk 4 in. in diameter, as it was found that when this was used idioretinal light caused considerable confusion. Accordingly a disk of light 1 in. in diameter was substituted, and it was found that greater confidence resulted. Since it was desired to study peripheral adaptation only, and to exclude any element of form perception, it was not practicable to use a field of more unmistakable shape, such as the triangle used by Allen & Dallenbach (1).

As finally arranged, the test field subtended a retinal angle of about 6°, and was displaced nearly 40° from the fovea. To ensure constancy in this latter respect, the subject fixated a spot of red light about 1 mm. in diameter, placed at eye-level. The handle controlling the diaphragm was operated by the subject, the instructions being to reduce the intensity until the test field just disappeared, and then to increase it until the light was just visible. This second point was taken as threshold value. In cases of doubt the subject was permitted to check his setting by further adjustment. Readings were generally taken at 5 min. intervals during the first hour and at 10 min. intervals during the second hour, though no attempt was made to adhere rigidly to this plan. Longer intervals were used during the second hour because it was found that at this stage the subjects experienced considerably greater difficulty in setting the field at threshold, and consequently took longer to do so. Time values quoted, however, always refer to the time at which the test light was switched on.

All the experiments were carried out with two subjects, alternating as experimenter:

A. Miss Florence MacNeill, research student and part-time assistant in the Department of Psychology, whose willing co-operation is gratefully acknowledged, and

B. The present writer, lecturer in Psychology in the University of Edinburgh.

Subject A has normal vision; subject B has congenital amblyopia of the left eye.

Following Nagel's practice, results were generally dealt with by drawing curves in which the ordinates represent reciprocals of threshold intensities (in the present case logarithms of reciprocals). These produce a rising curve, representing increase in sensitivity. Enhanced dark adaptation, if and when it appeared, would accordingly be seen as a further rise in the course of the curve. I have consistently interpreted my results in terms of logarithmic units, since most investigators are now agreed that this is desirable as being in accordance with the general response characteristics of the organism. A fuller consideration of the whole question of recording and interpretation of results will be given elsewhere.

Since differences occur both in rate of adaptation and in what may be termed its 'extent', i.e. final value of threshold stimulus intensity, it seems desirable to note both. *Extent* is here measured in terms of maximum level reached by a smoothed curve of adaptometer readings during a 2 hr. period, excluding rises at the end due to a single high reading. *Rate* is measured in terms of minutes taken for the smoothed curve to reach a given level or levels of sensitivity corresponding to a stated number of logarithmic units above the initial intensity of the adaptometer light. As mentioned later (§ V (4)), this smoothing of curves is not an entirely legitimate procedure, but it is useful or even unavoidable if one wishes to note results in the way just described.

III. DARK ADAPTATION FOLLOWING PRELIMINARY STIMULATION

(1) *The work of Kravkov and Semenovskaja*

As noted in §I, the immediate stimulus to the experiments here described was the results of an earlier series of investigations, those of Kravkov and Semenovskaja (5, 6, 9). Since the last of these papers provided the starting-point of the present study and since it is somewhat inaccessible to readers in this country, a brief account of its main features may be given here.

In the earlier papers (5, 6) it was claimed that monocular sensitivity in the dark-adapted state was enhanced by stimulation of the other eye, either before dark adaptation commenced or during its progress. The instrument used was devised by Kravkov, and may be described as a simplified form of the Nagel adaptometer. Three subjects took part in Semenovskája's experiments (9), all female, with normal vision, and between the ages of 21 and 27.

In general, the procedure was to stimulate one or both eyes with white or red light of 250 or 75 lux (about 23 and 7 f.c.) reflected from a white surface. A period of dark adaptation of approximately 2 hr. followed, readings being taken at intervals of from 3 to 15 min. Each subject carried out two or (usually) more sessions under each set of conditions. The results are presented in the form of (*a*) tables averaging the results for each subject separately, and (*b*) curves, in which the results for all three subjects are pooled. The results are quoted in units proportional to reciprocals of successive threshold intensities. Especially when presented in graphical form they show a very marked enhancement of adaptation (*a*) with preliminary red as compared with preliminary white stimulation, and with both as compared with sessions in which no preliminary stimulation was given; (*b*) following stimulation with light of 250 lux (of either colour) as compared with light of 75 lux. The differences are particularly marked for binocular adaptation.

Table 1. *Summary of results obtained by Semenovskaja* (9) *with and without preliminary stimulation with light of* 250 *lux*

	Sensitivity		
Conditions	At 10 in.	Maximum	At 120 in.
Preliminary red	4·90	5·68	5·04
Preliminary white	4·03	5·50	5·08
No preliminary	4·26	4·97	4·75

As presented, these results are very striking. Sensitivity values are plotted on an arithmetic scale, and in order to allow all three curves to be plotted on the same axes in manageable proportions, units have to be chosen in such a way that the 'normal' curve seems hardly to rise at all, while the curves for preliminary stimulation soar far above. If, however, the curves are replotted on a logarithmic scale, in the same way as those in the present paper, the differences are seen to be very much less striking. A summary of some of the results is given in Table 1. Each figure is an average of the results of all three subjects.

The actual figures of course depend on the characteristics of Kravkov's adaptometer, and on the whole they range somewhat higher than those obtained in my own series of experiments. It is interesting, however, that the initial values (not here quoted) are very similar to those obtained under comparable conditions by my subject A, who falls within the same category as regards age, sex, and visual characteristics. This would suggest that Semenovskaja's subjects appear to have achieved a somewhat further degree of adaptation.

(2) *Experiments on adaptation following stimulation with red light*

Even logarithmic plotting, however, leaves an impression of marked enhancement of dark adaptation following preliminary stimulation with either red or white light. (A brief discussion of this phenomenon is given in §V(2).) It was accordingly decided to attempt to confirm these results, either in detail, or, if this should prove impossible, in a more general way.

Table 2. *Adaptation with and without preliminary red stimulation*
(*Numbers in brackets indicate order in which the sessions were taken*)

	Maximum sensitivity attained	
Conditions	Subject A	Subject B
No preliminary		
1st session	4·47	4·30
2nd session	4·45	4·37
3rd session	4·51	4·46
Preliminary red		
Type (*a*)	4·50 (2)	4·36 (2)
Type (*b*)	4·43 (1)	4·42 (3)
Type (*c*)	4·48 (3)	4·37 (1)

Various types of red stimulation were tried:

(*a*) A white surface, as described on p. 138, viewed through red spectacles. For subject A a 40 W. white milk-glass bulb was used; for subject B a 100 W. blue daylight bulb. Semenovskaja's method was of this type.

(*b*) The same surface, illuminated by a 25 W. red Osram lamp, and viewed directly.

(*c*) The red-illumined surface, as in (*b*), viewed through red spectacles, as in (*a*).

Each subject did one set of observations with each type of red light, and by way of control, three sessions 'without preliminary stimulation'—except for 5 min. adaptation to the general illumination conditions of the test-room, which was lit with an ordinary 60 W. opaque bulb about 7 ft. above the floor. This represents a slight departure from Semenovskaja's practice, which involved 50 min. preliminary adaptation to a given illumination, but the completely negative character of my results makes it plain that the small differences in technique described are unlikely to have been of significance.

Table 2 summarizes the results of the sessions with and without

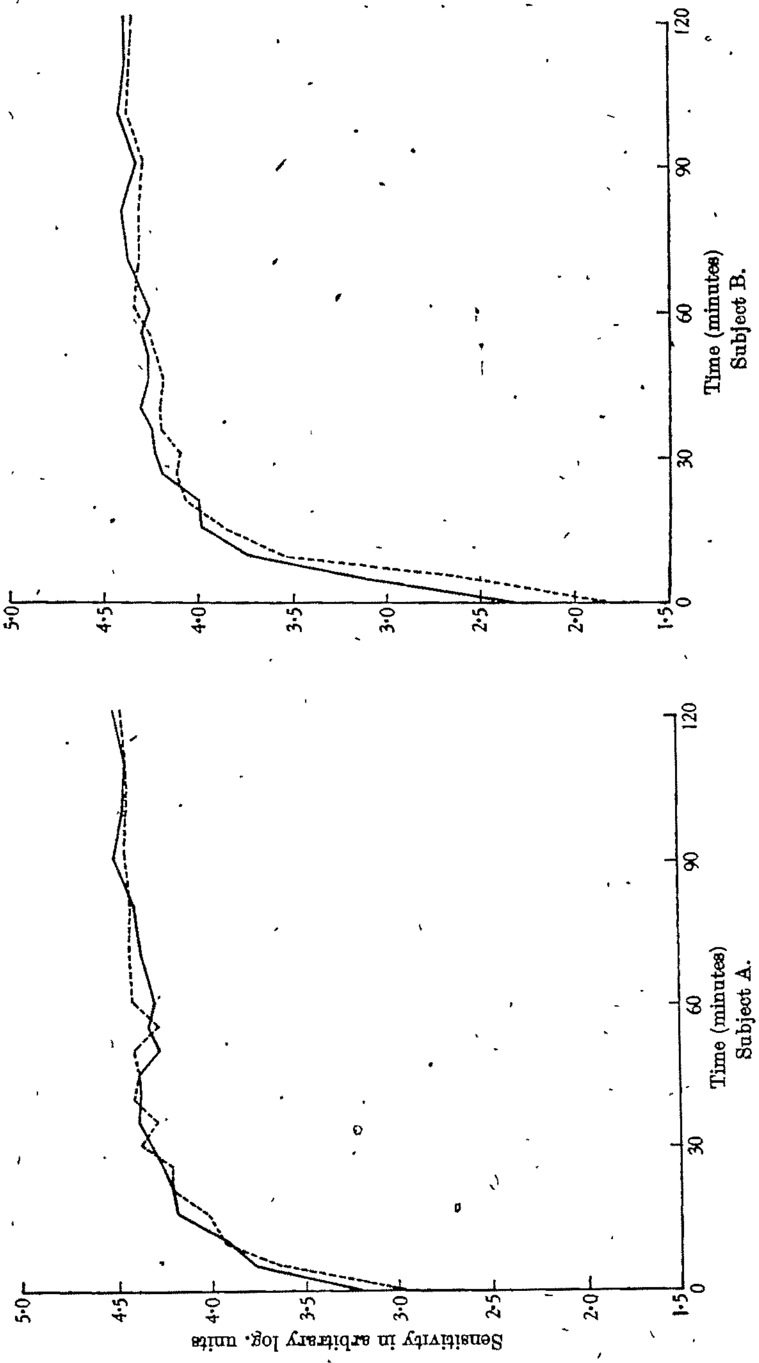

Fig. 1. Dark adaptation with and without preliminary stimulation with red light. Average curves.
——— with preliminary red - - - - - - - - no preliminary

preliminary red stimulation. That no effect was produced by stimulation with red light is still better seen in Fig. 1,[1] in which the results for all types of red stimulation are averaged for each subject, and compared with average curves for the sessions without preliminary stimulation.

(3) *Experiments on adaptation following interruption with red light*

A further variation of the same technique was tried as follows: Following 15 min. light adaptation, the subject was dark-adapted until adaptation seemed virtually complete; this took 75–80 min. Ten minutes stimulation with red light was then given, and a further series of readings taken during the remainder of the usual experimental period; this amounted to 25–30 min. The results are summarized in Table 3.

Table 3. *Summary of adaptation results before and after interruption with red light of two intensities*

Subject	Relative intensity of red light	30 min. immediately preceding interruption		Period following interruption		
		Mean sensitivity	Final reading	Initial reading	Mean sensitivity	Final reading
A	50	4·36 ± 0·039	4·37	4·34	4·37 ± 0·020	4·35
A	1	4·27 ± 0·044	4·35	4·28	4·23 ± 0·078	4·24
B	50	4·42 ± 0·063	4·55	4·20	4·39 ± 0·064	4·43
B	1	4·38 ± 0·070	4·52	4·47	4·40 ± 0·073	4·49

Since adaptation was in general still proceeding during the 30 min. preceding interruption, direct comparison of the dispersion of sensitivity measures before and after interruption cannot be made. It is interesting, however, that in three cases out of four this dispersion is as great as or greater than that caused by the series of rising readings in the fore-period. This fluctuation is in agreement with the observation of Lythgoe & Phillips (7) that any change of conditions during an experimental period brings about irregularity of readings. The other facts that emerge are that the drop in sensitivity caused by the interruption is small, and that after about 30 min., if not before, dark adaptation has nearly if not quite reached its former level. No significant or regular difference seems to exist between the effects of the two intensities used.

(4) *Conclusions regarding preliminary stimulation*

General conclusions from all these experiments seem to be as follows: (1) There is no evidence of the enhancement of dark adaptation through stimulation with red light noted by Semenovskaja. (2) A certain improvement in sensitivity, corresponding to ordinary dark adaptation, does, however, occur *during* such stimulation. In other words, adaptation to some extent takes place under these conditions. As will be seen from the initial portions of the curves in Fig. 1, this occurs more rapidly than in the presence of even fairly dim diffused artificial light, and the results in Table 3 show that the loss in sensitivity ensuing from stimulation with red light while in the dark-adapted state is relatively small.

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[1] In all figures ordinates represent logarithmic 'sensitivity units', zero being the maximum intensity of the adaptometer.

IV. DARK ADAPTATION DURING CONTINUOUS STIMULATION

The above conclusions led to the study of the main subject of this paper: how far it is possible to achieve normal dark adaptation during continued exposure to coloured light. This is a matter of considerable practical interest, and in order to obtain information on conditions which might conceivably occur in practical situations, two different types of light stimulation were tried, neither being that of the previous experiments: (1) diffused coloured light of various intensities, from a source above and behind the subject, and (2) 'point' lights from an 'Ever-ready' cycle lamp, placed in the subject's field of vision, in a position approximating to that of a motor dashboard. In all cases the light was kept on throughout the session, but was switched off to enable the subject to make settings on the adaptometer.

(1) *Diffused light*

Two colours were originally used in the first series of experiments—the red lamp already used in the experiments described in §III (2) and (3), and a similar blue lamp. The intensity of the stimulating light was varied, as in the case of the 'interruption' experiments, by means of a rheostat, the extremes being in each case in the ratio 50 : 1. These intensity values are of course relative to one another only for a particular colour; i.e. intensity '50' (the maximum intensity for each lamp) for the red light had not the same brightness or illumination value as intensity '50' for the blue. The relations of the various intensities of the different coloured lights is discussed below, and in §V (3*a*). Later, one subject (B) did a series of three sessions with orange light.

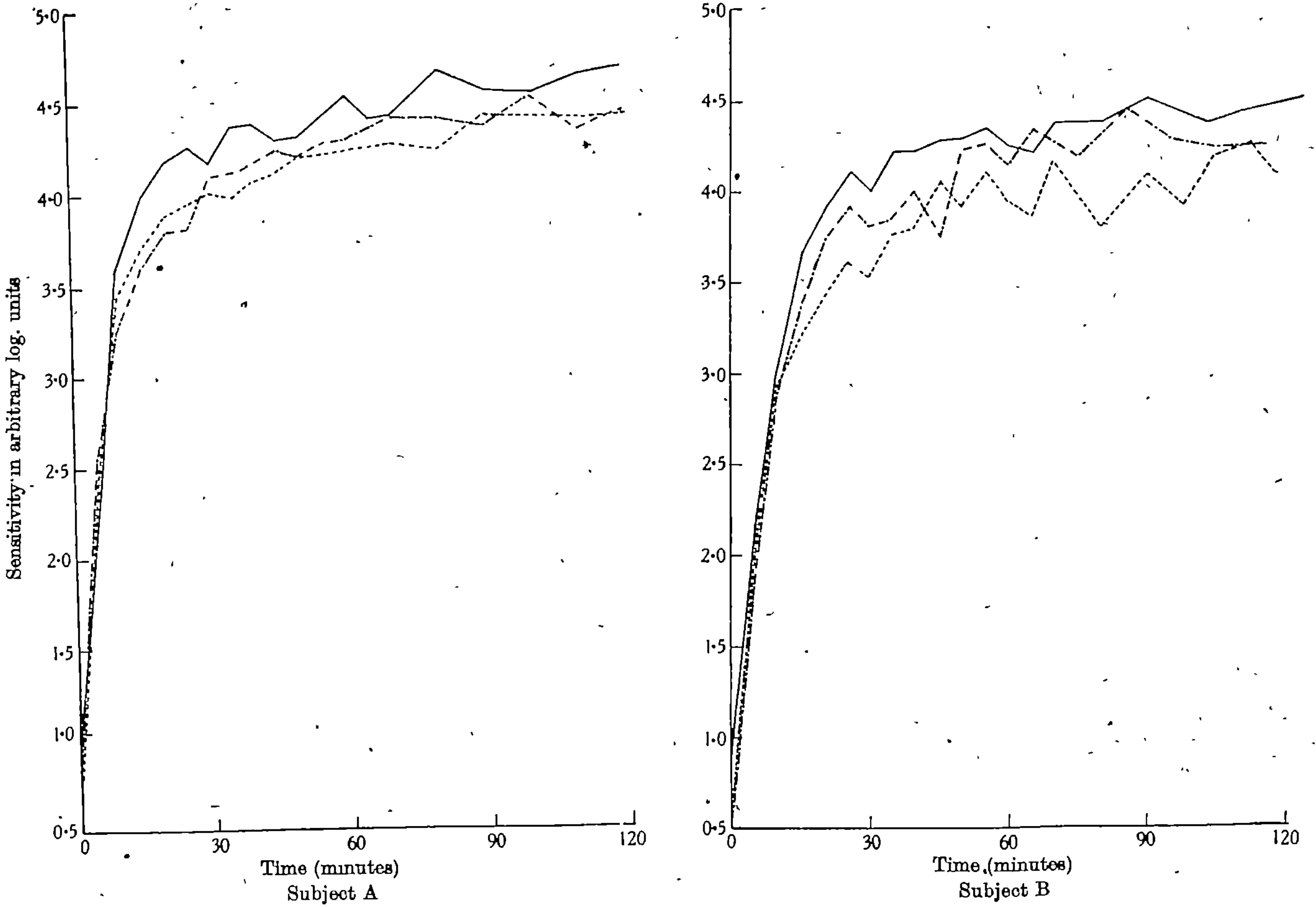
Detailed results are given in Table 4 below. In Fig. 2 curves of composite results for each subject are shown. These represent the average of results obtained with two intensities ('50' or '25' and '1') for subject A, and with three ('50', '10' and '1') for subject B. The two sessions with blue light of intensity '1' (see Table 4) are themselves averaged in obtaining subject A's curve for blue, and the anomalous result obtained in one of the sessions with red light of that relative intensity is omitted. This result, noted in square brackets in Table 4, was obtained from a session which took place after a break of about 7 weeks during the summer vacation, during which time no experiments were carried out. Effects of practice, or lack of practice, were also observed in other connexions, and will be discussed elsewhere. For a time I also thought that subject B's results for orange light might have been affected by the fact that these sessions took place at a different phase of habituation to the test situation, but the results for red light noted in Table 4 are almost identical with those of the first 2 hr. of a prolonged session with red light of the same intensity ('50')—described in another paper (10) and not included in the table—which took place much closer in time to the sessions with orange light.

Table 4. *Extent and rate of adaptation during continuous stimulation with coloured light. Rate measured in minutes to reach sensitivity levels* 2, 3 *and* 4

Colour	Relative intensity	Extent		Rate	
		Subject A	Subject B	Subject A	Subject B
			Diffused light		
Red	50	4 40	4·25	4– 8–25	6– 9–46
	10	—	4 28	—	6–11–26
	1	[4·39]	—	[4– 9–33]	—
		4·67	4·41	4– 8–16	4– 8–19
Blue	50	—	4·18	—	4–11–54
	25	4·32	—	4–15–29	—
	10	—	4·13	—	4–11–40
	1	4·54	4·31	4–11–18	5–11–27
		4·66	—	3–11–19	—
Orange	50	—	4·38	—	6–10–30
	10	—	4·43	—	6–10–24
	1	—	4·54	—	5– 8–17
			Point lights		
Red		4 60	4·44	4– 9–18	5–11–28
Green		4·38	4·31	4–10–30	6–12–47
Yellow		4 38	4·20	4–10–33	6–13–53
			Ordinary conditions (average)		
		4·65	4·47	4– 8–17	5–10–22

Without particularizing for the present, it may be said that extent and rate of adaptation show a good degree of inverse correspondence with intensity of stimulating light of a given colour.

Comparing the curves, however, it will be seen that both subjects show the same slightly better degree of adaptation with red as compared with blue light. It will further be seen that B's results with orange show still more complete adaptation. These results provided something of a surprise, since the red and orange lights were considerably brighter than



Subject A

Subject B

Fig. 3. Dark adaptation during continuous stimulation with coloured point lights.

——————— Red - - - - - - - - - Yellow —·—·—·—·— Green 'dark' being defined, as in Warren's *Dictionary of Psychology*, as "characterized by low brilliance, low illumination, or low brightness". With this definition, then, one can legitimately speak of dark adaptation as occurring during conditions of incomplete darkness or low illumination, such as those described in this paper. Besides being regarded as a process, dark adaptation may also be considered as a state, complete dark adaptation being the end-state corresponding to maximum retinal sensitivity. Considered in this way, there can be objection to the concept of fluctuations in the dark-adaptation process, or that of falling off towards the end of an observation period. My experiments have yielded instances of both these phenomena, the former in almost every session, however short, the latter only occasionally, and not to the extent noted by Kravkov & Semenovskaja ((6), (9), and see Table 1). In fact, as noted elsewhere (10), it was found that subject B was able to maintain dark adaptation unimpaired for a period of at least 6 hr., and there seems no reason to believe that this should not apply to any practised observer. A full discussion of this and other subjects raised in this section will appear in another paper.

Following the definition quoted, therefore, the present discussion is not primarily concerned with theoretical considerations which might furnish a physiological explanation of the phenomena observed. A certain amount of fairly convincing interpretation on a psychological basis may however be put forward.

(2) *The effects of preliminary stimulation*

Such classical authorities as Aubert and Nagel are quoted (by Allen & Dallenbach (1) and others) as expressing the view that dark adaptation is facilitated by the occurrence of light flashes, a fact which they claim can be readily verified in ordinary experience. Although this claim lacks experimental confirmation, there is a sense in which such facilitation does seem to take place, since, as shown by Allen & Dallenbach (1), Suchman & Weld (11) and others, temporary decreases in sensitivity caused by the interruption of the adaptation process by means of light flashes are compensated in such a way that the curve catches up, as it were, on itself, so that the time taken to reach a given level of sensitivity is the same as it would have been had no interruption occurred. This would explain the crossing of the curves of adaptation with and without 'preliminary stimulation' (see Fig. 1), but there appears to be no evidence, other than the results of Kravkov and Semenovskaja, of dark adaptation proceeding so much further following any sort of stimulation of different colours. It is suggested that in addition to relative visibility factors certain affective influences must be taken into account.

5. Throughout the investigation stress has been laid on the psychological aspects of the dark-adaptation process; i.e. the subject's experience of the test situation. It is shown that in addition to variations in extent and rate of adaptation, individual differences occur in respect of the regularity of its course. Results may also be considerably affected by practice and by day-to-day variation.

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EXPERIMENTALLY INDUCED OLFACTORY PARAMNESIAS

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I. INTRODUCTION

We described in an earlier paper (1) a method of inducing paramnesia under experimental conditions. It was shown that visual material given under hypnosis, and associated with post-hypnotic amnesia, evoked a paramnesic variety of recognition when given on the following day in the waking state. These experimental paramnesias closely resembled the states of consciousness commonly associated with incomplete or arrested recognition. It was suggested that they threw some light on the origin of paramnesia in daily life.

We stated in our earlier paper that odours as well as pictures were used in the experiment. Odours were chosen in preference to auditory or tactual stimuli largely on account of the frequency with which they are said to be associated with paramnesia in daily life. We anticipated, of course, that it would be more difficult to obtain conclusive results with always recorded. In some cases, additional presentations of the stimuli were given at the end of the series. An interval of 2 to 4 min. was allowed between each stimulus. Responses were taken down *verbatim*.

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In the case of one subject (M), each session was conducted by a different experimenter. Dr J. M. Blackburn, who kindly presented all the material for recognition on the third day, had no previous information about the construction of the test series.

III. RESULTS

(1) *Subject W*

The responses given to Series *AO* during Waking Session I will be considered first; then those given to Series *BO* during the Hypnotic Session; and lastly, the reactions given to the three sets, *AO*, *BO*, and *CO*, during Waking Session II.

Waking Session I. Series AO. One of the three odours was correctly named. One was described carefully, and a possible name suggested. One gave rise to a definite paramnesia, in the sense in which this term has been defined in our earlier paper. It was not of course conditioned by our hypnotic procedure.

AO, 1: *Nitrobenzene.* "I should say nitrobenzene. Partly because it's slightly different from prussic acid, and partly because I didn't think you'd use prussic acid!"

AO, 2: *Valerian.* "Can't get it. An oil of some sort. Smells rather heavy and sweet. I seem to have smelt it before but can't trace it down to anything."

AO, 3: *Aniseed.* "Smells a little like some of the things they mix for cough mixtures—don't know what it is. It's something dissolved in alcohol. Is it chlorodyne?"

The odours were then given again and the subject told their names in the usual way. He accepted all without question.

Hypnotic Session. Series BO. The following responses were given to these three odours:

BO, 1: *Capronic acid.* "Don't know. Something sour....Can't say. Some sort of cheese or something."

BO, 2: *Creosote.* "Don't know what it is. It smells like a chemist's shop...form of antiseptic sort of stuff. Smells spicy, rather."

BO, 3: *Thymol.* "Smells rather like the second, but rather heavier. A little bit like oil of wintergreen—but not quite."

The odours were given a second time together with their respective names, and the post-hypnotic suggestions of amnesia were given as described.

Waking Session II. The responses given during Waking Session II to Series *CO*, *AO*, and *BO* will be considered separately.

Series CO. One of the six odours was recognized and given its correct name. Three were described in detail and possible names tentatively suggested in each case. This naming was of a trial-and-error character, obviously based on analogy. One odour was correctly treated as a member of the general class to which it belonged and aroused some recognition. One odour elicited a distinct paramnesia—in the sense of an impression of familiarity with inability to recall the required name.

We may give some illustrations of these responses:

(8) *CO*, 4: *Camphor.* "Camphor, isn't it?"

(3) *CO*, 2: *Juniper.* "Smells like pure resin—generally tarry, resinous. Pine idea—like a pine shampoo. Anthracene?"

(12) *CO*, 6: *Verbena.* "A citronish sort of smell—probably citric acid. Very lemon smell. Smells rather sweet."

(10) *CO*, 5: *Perfume.* "A common or garden scent of some kind. I don't remember the name. I think it must be fairly popular. I've smelt it quite a lot before."

(1) *CO*, 1: *Vanilla.* "Can't trace it. I can't get the name. It's the smell of a certain kind of chocolate—an essence of something."

Series AO. One of the three odours in this series, nitrobenzene, was correctly named and related to the actual session on which it had been given. Recognition was rapid and complete. One of the odours was said to be "like aniseed" when it in fact was aniseed. It was not said to have been given on an earlier session. Recognition was thus incomplete, since reference was not specifically made to the earlier presentation. The third odour gave rise to a response with obvious paramnesic elements. The subject thought it unlikely that he had been given this stimulus on the first day of the experiment. These three responses are given in full for purposes of comparison with those to the odours given in the Hypnotic Session:

(2) *AO*, 3: *Aniseed.* "Rather like aniseed. Smells like some form of medicine—a spicy sort of smell."

(7) *AO*, 1: *Nitrobenzene.* "Nitrobenzene: the first on Monday (the first day of the experiment). It always reminds me of furniture polish of a certain sort."

(9) *AO*, 2: *Valerian.* "One I've smelt recently. No, I don't think on Monday—it may have been. I can't name it."

These three odours were given again at the end of the whole series and the subject was told their respective names. He then recalled that the aniseed had been given on the first day. He also recalled the name 'valerian', but had failed to associate it with the requisite stimulus.

Series BO. The three odours in this series gave rise to responses of a distinctly paramnesic type. The paramnesia was quite immediate in two cases, but rather less so in the third.

(2) *Subject M*

We shall consider in order the responses given on the three successive sessions of the experiment. The responses made on the third day during Waking Session II are given in full.

Waking Session I: Series AO. This subject responded in a very full and discursive manner to all three stimuli. We quote extracts from his accounts of each odour:

AO, 1: *Nitrobenzene.* "Burnt almonds. Reminded me of all the ways of eating almonds, particularly of eating them in Germany.... Also of war gas...."

AO, 2: *Valerian.* "Reminded me of an operation on my foot. Heat of the room and oil put on my foot. I remember the nurse behind me telling jokes and the surgeon being irritated...."

AO, 3: *Aniseed.* "I think it is used by doctors to make medicine palatable to children.... Doctor arrived by car to treat me for scarlet fever. Eaten sweets called gob-stoppers... lasted through long cinema performances...."

There is nothing of note in these responses other than their prolixity. Certain of the reminiscences, however, appeared again during the recognition test on the third day.

Hypnotic Session: Series BO. The responses made by this subject under hypnosis are given below. It will be recalled that hypnosis was moderately deep.

BO, 1: *Capronic acid.* This stimulus called forth a dramatic reaction. The subject uttered in a very solemn tone of voice the phrase: "Just *Leichengift*." When asked to explain what he meant, he became very agitated and started to talk rapidly in a low voice. We were able to detect the words: "*Leichengift*—it is the smell of dead soldiers." He then described an experience which he had undergone during which he had been in contact with corpses in a state of putrefaction. We believe that this experience was genuine, though we have no definite evidence on the point.

BO, 2: *Creosote.* "Reminds me of the operating theatre and all the volatile odours one has there: heat, steam, water-bandages... light from the globe; noise from the sterilizing apparatus...."

BO, 3: *Thymol.* "I couldn't tell you what it is but I'm sure we use it when we have sprains... when you have a strained wrist—from running too hard. It may be a burn dressing too."

None of these three odours was identified by name, though all seemed to possess a striking redintegrative value for this subject. The reaction to the first was especially interesting, and, as will be seen, had repercussions on the recognition test the following day. As usual, the three odours were given a second time during hypnosis and the subject was told their names.

Waking Session II: Series CO. One of the six new stimuli was named correctly. Three could not be named but gave rise to redintegrations of the general type described. One was confused with an odour given during the first session, and one gave rise to a definite paramnesia. Some extracts from our protocols will serve to illustrate these different types of reaction:

(8) *CO*, 4: *Camphor*. "I am sure I have smelt that before. It is camphor. It is something that played a large part in the first years of growing up...."

(1) *CO*, 1: *Vanilla*. "That reminds me of a certain sweet we used to eat as children....I can remember the colour of the sweets. They were pink or blue and smooth as an egg...."

The following odour was confused with *AO*, 3 (aniseed). This was probably due to the very faded preparation of civet which we used and not to defective discrimination on the part of the subject.

(5) *CO*, 3: *Civet*. "Yes, I'm afraid I haven't got a good nose for things, especially if I have to smell one thing after another. But I am sure it formed part of the sweets we used to eat. I think we had that on Monday. I think I told your colleague it was a thing called a 'gob-stopper'."

The last odour in the series gave rise to an indubitable paramnesia of recognition. It must be borne in mind, however, that this odour was given directly after *BO*, 3, thymol, which induced one of the most striking paramnesias in the whole series. There may therefore have been some contamination of this stimulus by its predecessor.

(12) *CO*, 6: *Verbena*. "Dear me! What a poor memory I must have. This is most irritating. I am sure I have smelt that before. I wonder if I smelt that on Monday? (the first session).—I don't know—I am sure I know something connected with it. My mind seems to be very blank. I am sorry, I can't tell you. The trouble is I can't really say whether I smelt it on Monday now. No, I can't fix that."

This response betrays all the features which we have decided to treat as paramnesic. But we are not aware that the subject had experienced the odour before, though it is certainly fairly common. It was not especially similar to any of the *BO* odours given during hypnosis.

Series AO. None of the three stimuli in this group was identified in the ordinary sense of the term. It therefore follows that the reactions to the *BO* odours are less well controlled than those to the corresponding pictures discussed in the previous paper. But a distinctly paramnesic reaction was given to only one of the three *AO* odours, whereas all the *BO* odours elicited responses showing a more marked element of paramnesia. It is convenient to describe the two non-paramnesic responses here, leaving the third to be considered along with the responses to the *BO* odours.

stimulus itself, nor of the fact that these associations had been given almost the same words on the previous day. We cannot call this a case paramnesia. Rather must it be classed as an amnesia—as an *absol* recognition failure for hypnotic material.

The third odour in this series was thymol. In this case, the respon was markedly paramnesic, and it also throws some light on the mechanis of hypnotic inhibition.

> (11) *BO, 3. Thymol.* “I wonder where I have smelt that before? Perhaps I sm it when we were in the workshop down below before coming up here. And I hav feeling that as soon as I get home I shall remember something else about it too. N I'm sorry, I am surprisingly (dense). I feel I have got something to tell you abou too, but I can't get it fixed. I can see a word in my mind and I just can't say it. is—er—I keep asking myself why I don't hurry up, and say it, but (*laughs*)—the w is 'yesterday'—(*very loudly and distinctly*)—That was quite an achievement, I sure!” *What was?* “Saying that. I saw the word as clearly as I see that gra over there and I saw it in my mind.” *Why couldn't you say it?* “I couldn't bring into any sentence....I feel exactly like a small boy who keeps on raising his hand say something and the teacher never looks at him.”

This reaction, besides having all the characteristics of local para nesia, is marked by the intrusion into consciousness of a presumab dissociated imaginal response. At any rate, this image must have be the outcome of the unconscious recognition process. The reaction as whole shows the following stages: (1) The subject at once asked hims where he could have smelt the odour before, thus betraying an act reference to past experience. (2) He first answered his own question reference to the odour of the workshop which he passed on his way to t room in which the experiment was being conducted. This was probab construction of a rather facile character, for no odour of this kind w present in the workshop itself. (3) The subject then described a certa difficulty in recalling data which he believed to be connected with t thymol. (4) There was an intrusion into consciousness of the wo 'yesterday' carried in visual form. (5) There was considerable difficul in communicating this word to the experimenter, and it is interesti that the subject found it difficult to include the word in any senten (6) The subject appeared fairly satisfied with his effort of recall. On t other hand, he did not betray any awareness of the significance of t word produced with so much labour. He did not go on to suggest a connexion between it and the events of the previous day. It is possil that he was in some degree aware of the connexion but he was entire unable to state it.

A short summary of the results obtained from Subject M on the third day is given below:

(1) One new stimulus was named correctly and three were named by analogy with more familiar odours. One stimulus was tentatively ascribed to the Waking Session I, and one gave rise to responses of the paramnesic type.

(2) Three of the six *CO* stimuli aroused reactions of a redintegrative character. Some of these went back to situations of childhood and early life.

(3) None of the *AO* odours was identified. One of the three was regarded as completely new. The second called out responses similar to those given at the earlier session, but without recognition. The third was associated with an odour given during hypnosis which had elicited a marked paramnesia.

(4) Two reactions to *BO* stimuli were strongly paramnesic. In the first, there was good evidence that the hypnotic inhibition was operating in conjunction with a pre-existing repression. In the second, a visual image suddenly appeared in consciousness which carried some reference to the hypnotic session. But the subject certainly lacked insight into the significance of this image.

(5) The third *BO* stimulus aroused redintegrative reactions almost identical with those given the previous day under hypnosis. There was no trace of recognition in this case.

(6) A certain similarity between the results of our hypnotic procedure and normal paramnesias are indicated though our control with the odours is obviously less exact than it was in the case of the pictures.

(3) *Subject P*

The results obtained from this subject are presented in the usual way.

Waking Session I: Series AO.

AO, 1: *Nitrobenzene.* "Immediately thought of the chemistry labs. Some aromatic organic substance—not aniline. I was immediately transported to the chemistry labs."

AO, 2: *Valerian.* "Reminds me partly of a chemist's shop, and before that of moth-balls and a fur coat...."

AO, 3: *Aniseed.* "Once again I thought of the chemistry labs. Also reminded me of oil of wintergreen—and tooth paste. The sort of smell that hangs around on tooth glasses and the wash basin."

The three odours were given a second time in the usual way and the subject informed of their identity.

unable to formulate these facts. Subject M, for example, showed considerable insight into his perplexity. But in general, the nature and course of experimental paramnesia was the same with both visual and olfactory stimuli, and it is probable that it would be found to be the same whatever the type of stimuli with reference to which it was set up.

We have recorded two somewhat atypical olfactory paramnesias which merit a short discussion. These were obtained from Subject M in responding to two of the odours given under hypnosis. It will be remembered that this subject associated one of these odours, namely capronic acid, with a traumatic episode in his military experience. There was also evidence that this episode had been in part at least repressed. On the third day, capronic acid gave rise to an outstanding paramnesia which the subject at first found himself totally unable to interpret. Eventually he succeeded in relating the odour to the experience in question and the paramnesia was apparently dispersed. But there appeared to be no connexion in the subject's mind between the smell of capronic acid and the experiences of hypnosis. It would seem that we have in this case two repressive factors, arising at different times and for different reasons and reinforcing one another in producing a certain type of response. Situations of this kind raise interesting problems connected with the study of repression.

The second atypical paramnesia occurred when Subject M reported the appearance in consciousness of the word 'yesterday' on being presented with thymol. It is interesting that the subject experienced great difficulty in telling the examiner what the word was and in introducing it into a sentence. Even when he succeeded in communicating the word, its true significance was apparently lost upon him. At any rate, he made no explicit reference to the events of the preceding day. This would seem to indicate that the *meaning* of the word, in so far as it bore on the problem confronting the subject, remained dissociated. More concretely, the subject lacked insight into the relation of his imaginal response to the precipitating stimulus. It follows that the mere appearance of a relevant image in consciousness need not be sufficient to provoke recognition. This result may be of interest to those who regard recognition as dependent on the revival of discrete images. At any rate, we have a fairly clear example of the way in which a subject may lack insight into the significance of an image even when this image can be subjected to introspective scrutiny.

We may pass on to a short discussion of affect in paramnesia. It may be recalled that MacCurdy, in his general discussion of *déjà-vu* (6), holds that experiences of this character are invariably associated with an unpleasant affect. The affect arises, he believes, in two ways, (*a*) from the failure to resuscitate the details and setting of the earlier experience, and (*b*) from the compulsive character of the impulse to try to do so. We certainly noted that the phase of perplexity in experimental paramnesia might be extremely disconcerting, and in some cases markedly unpleasant. There was but one case, however, in which the reaction could be said to be definitely emotional (Subject M, to *BO*, 1). And in this case the unpleasant affect was undoubtedly determined by an underlying complex. In the remaining cases, the dominant affect was one of perplexity, mildly irritating, and doubtless arising from MacCurdy's two sources.

It is obvious that our experimental paramnesias occurred under conditions in which recognition was impeded by a process akin to repression. But we do not wish to imply, nor does it seem justifiable to assume, that repression must always precede the appearance of paramnesia. We have accumulated a fair amount of evidence (not yet published) that this phenomenon may be associated in a large number of cases with the perception of objects in unwonted or unexpected settings. As an example of an olfactory paramnesia arising in this way, we may quote a report kindly furnished us by a Cambridge anthropologist (Dr E. J. Lindgren). The similarity of her experience to the phenomena provoked under our experimental conditions is self-evident:

> I was recently driving Mr and Mrs L. from my house into Cambridge when I became gradually aware of the most delightful sensation, and presently identified it as caused by a smell. This seemed so exceptionally overwhelming in its pleasantly drug-like effect that I felt obliged to attend further to it and to try to give it a name. Suddenly it came: 'Mongolia.' There was just time for me to think: "But L. cannot *smell* of Mongolia when he left it nine months ago", when I exclaimed: "Oh! What *have* you got with you?", and turned round to find him smoking a pipe and hear him explain that it was in fact Mongol tobacco.

The altered setting of the stimulus in the above example is probably sufficient to account for the striking paramnesia. It is obvious that the odour in question would have been recognized immediately had it formed part of its accustomed setting. Indeed in this case, as in many similar cases which we have recorded (though not yet published), the primary obstacle to recognition is bound up with the necessity for a certain reorientation. There is difficulty in changing from the prevailing orientation to the one demanded by the situation in which the intruding element is normally to be found. In such cases, the incongruous element does not provide a sufficient stimulus for the readjustment demanded. The resulting paramnesia expresses both the need for a reorientation and the difficulty in bringing it about. It is at the same time a challenge and a confession of failure.

We cannot enter here into a full discussion of orientation and its significance in recognizing. But we should like to emphasize the usefulness of the general idea and the part which it has played in the theory of memory. Betz (3), the first to think along these lines, founded his notion of *Einstellung* on an analysis of paramnesia. Claparède, in an unduly neglected contribution (4), has given an analysis of paramnesia very much along the lines of our present work. In more recent times, Bartlett (2) has laid particular emphasis on the orientation factor and its role in recognition. MacCurdy (7), and after him Koffka (5), devote some discussion to the same problem. In general, we think it correct to state that responses of the paramnesic type are forthcoming wherever a perceptual pattern fails to call out the orientation appropriate to it. This may result from a number of causes. In the example given above there was a genuine conflict with the prevailing orientation. In our experiments we have to deal rather with an artificial check imposed by the post-hypnotic inhibition. But in all cases paramnesia expresses the need for an active readjustment of attitude. Its functional significance is indeed obvious.

The relation of our earlier results to *déjà-vu* was indicated briefly in our first paper. Since that paper was written, Dr R. W. Pickford has introduced us to some material which suggests that the connexion between the different types of paramnesia may be closer than we had suspected. We hope to deal in a later paper with the relation of our results to Dr Pickford's recent work (8) and to the general theory of memory.

VI. SUMMARY

1. Illustrations are given of restricted paramnesias set up in response to olfactory material in three subjects.
2. The paramnesias were most common with material for which post-hypnotic amnesia had been induced on the previous day.
3. Control of the hypnotic variable was less complete than in the experiments with visual material. The results nevertheless indicate a definite association between the inhibitions given under hypnosis and the appearance of paramnesia during the recognition test.
4. The paramnesias resembled in their general character those set up in response to visual material under identical experimental conditions.
5. Two atypical paramnesias are described which illustrate certain aspects of repression.

6. Affect in paramnesia is briefly discussed.

7. The relation of paramnesia to the orientation factor in recognizing is considered. The significance of this factor for effective reaction is emphasized.

Our thanks are due, and are tendered, to all those who helped us in these experiments, in particular to Drs J. M. Blackburn and K. J. W. Craik; and to Dr R. W. Pickford for some very interesting suggestions to which we hope to do full justice in a subsequent paper.

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PUBLICATIONS RECENTLY RECEIVED

Organizing and Memorizing. By GEORGE KATONA. New York: Columbia University Press (London: Humphrey Milford, Oxford University Press). 1940. Pp. xii+318. 23*s.* 6*d.* net.

This book consists mainly of a detailed account of a most ingenious series of experiments designed to show the much greater speed and efficiency of learning by 'understanding' of a situation, in this case the apprehension of the essential features of a series of problems, than by rote memorizing of the solutions of such problems. Moreover, the understanding gained by the first method of learning can be transferred effectively to other similar situations, whereas memorized solutions give little if any assistance in dealing with other similar problems. Although the problems investigated are verbal and arithmetical, the author shows that understanding which is adequate for solving the problems can take place without the necessity of fully formulating the method of solution. This is an important conclusion, since it indicates the possibility of a transfer, through such an understanding of the situation, in the acquisition of skills other than verbal. The author does not deal with such skills, but it would be of the greatest interest and value if he could extend to their study his admirable technique and experimental design.

On the theoretical side, he tries to link up his results with Koffka's trace theory of remembering. This part of the argument is less convincing. But the important point is made that there are not two fundamentally different learning processes, (*a*) the learning of meaningful material, (*b*) the memorizing of meaningless. But there are differing degrees of organizing, with a maximum amount in learning through 'understanding', a minimum amount in rote memorizing; no learning occurs unless there is some organizing, e.g. in rhythmically arranged groups.

This most interesting and valuable study of the learning process is highly recommended to all psychologists.

The Psychology of the Interview. By R. C. OLDFIELD. London: Methuen and Co., Ltd. 1941. Pp. xv+144. 5*s.* net.

This book should perhaps be more accurately entitled 'The Psychological Analysis of the Interview', since this task of analysis it performs most admirably. It was indeed opportune to point out that an interview is not an interchange of stereotyped question and answer, followed by the formation of an equally stereotyped assessment in set terms of the nature of the candidate's personality or fitness for an occupation. Mr Oldfield stresses the fact that the interview is essentially an interplay of attitudes between interviewer and candidate. The former, by a manipulation of his own attitudes (of which he is usually only partly aware), stimulates the latter to react by corresponding changes of attitude. From these the interviewer is able intuitively to formulate his assessment, apparently by creating in his own mind, and then observing and judging, a kind of picture or 'schema' of the candidate's personality as a whole.

This last conclusion is a particularly interesting one, and seems to tally well with Reik's description, in *Surprise and the Psychoanalyst*, of the temporary introjection of the patient's ego into that of the analyst. But Mr Oldfield does not sufficiently discuss the essential difficulty of this process, which is: The 'schema' does indeed take the form of a living 'homunculus', characterized by feelings and action tendencies which cannot be formulated in verbal terms. Thus the interviewer may be able to say to himself, "I know what sort of a man you are", while groping in the dark for words and concepts in which to describe the candidate's personality. Of course if the interviewer's task is merely to choose someone to fill a particular job, he may be able to judge the candidate's suitability intuitively and immediately, and the problem of verbal formulation does not arise. But it is disappointing to the psychologist that it should be left so much in the air; and that Mr Oldfield has been unable to make use of his experimental material to elucidate it further, and to investigate the validity of such verbal character assessments as can be made.

He must, however, be congratulated on his analysis of the interviewing process, and upon the lucid and attractive manner in which he has carried it out.

Human Nature and the Social Order. By E. L. THORNDIKE. New York: The Macmillan Co. 1940. Pp. xx+1019. 18*s*. net.

This monumental work, of over 1000 pages, gives as exhaustive a review as is possible, viewed from the behaviourist standpoint, of human nature and human institutions. It begins with the behaviour of the individual, his abilities and potentialities, his wants, needs and his ways of satisfying them, and his values. These findings are then applied to the study of social constructions, customs and institutions: social welfare, economics, industry and commerce, finance, politics, law and government. No more thorough treatment of the more superficial psychological aspects of these topics could be possible, and no one approaching them with the behaviourist outlook could go further. Some readers will be interested and instructed, some will find the book unreadable; yet others may lay it down saying rudely, "So what?" But many will, I think, conclude that it marks the limit of the value of psychological atomism, and of the scientific rationalism and humanitarianism which the author advocates, not realizing that the rise of Nazism (which he ignores) has disclosed their bankruptcy. Henceforth, unless there is some attempt to integrate isolated phenomena, to gain some general understanding of them and explain what they 'mean' to the experiencing individual or social group, psychology will soon become as remote from reality as mediaeval scholasticism. M. D. V.

The Nineteen Forty Mental Measurements Yearbook. Edited by OSCAR KRISEN BUROS. Holland Park, New Jersey. 1941. Pp. xxi+674.

Many readers of this *Journal* must have found occasion to consult the *Nineteen-Thirty-Eight Mental Measurements Yearbook*, an enterprise in publication which deserved, and apparently, achieved wide publicity and as wide appreciation. Now comes the Nineteen Forty issue, larger, more ambitious, and definitely better than the earlier volume. The amount of devoted labour which Prof. Buros must have put into this publication must have been prodigious. As should now be very well known, the general plan is to give a really comprehensive survey of tests and articles and books about tests which have been published during the period under survey. Not only is the material dealt with listed, with authors' and publishers' names and dates and with prices wherever possible, but a very excellent collection of published critical studies of the material is reproduced. The reader is therefore in a very much better position than if he merely had a summary, and nothing else, for deciding whether the tests or discussions dealt with give him what he may be trying to find. No reputable psychological library ought to be without this book, and there can be few teachers of psychology who will not be glad to be able to direct their more intelligent students to its pages. More than that, the worker in almost any applied field of psychology, who is himself searching for tests of practical value, will find here a mine of most valuable information. It is clearly one of the first essentials, in any volume of this kind, that the arrangement of sections should be lucid and systematic and the indexing very good. It is difficult, in this case, to see how these matters could be done better than they are. It is probably inevitable that there should be some omissions, but the present reviewer who has reasonably good opportunities for becoming aquainted with current work in at least some of the sections covered by the publication, has never looked into these sections without finding much to learn. It is very much to be hoped that the publication will continue for many years to come.

Psychology in Education. By HERBERT SORENSON. London: McGraw-Hill Publishing Co. 1940. Pp. xiv+489. 19*s.* net.

This is definitely intended as a work-book for students. Most of the chapters begin by directions about how to study the topic dealt with, and are followed by Problems and Exercises, References, and Additional References. A very large amount of ground is covered, and as the discussions are largely factual, the specialist will find lots of omissions and not a little dogmatism. It is probable that English psychological educationists who use the book will find it satisfactory only with their better, or more advanced, students, and will prefer to use it in conjunction with critical lectures. At the same time the writing is extremely clear and definite, and where experimental facts are reported in detail the reports are reliable. The author has rather a *penchant* for the individual illustration or story, and this may be all to the good, since it may do something to induce the student to treat his future pupils as individuals and not merely as so many types of reaction.

The Three R's and the Adult Worker. By K. E. NORRIS, with an Introduction by C. E. KELLOGG. Montreal: McGill University (London: Humphrey Milford). 1940. Pp. xiv+213. $2.00.

It would be a good thing if something like this Canadian study could be repeated in this country. Dr Norris was interested in how much of his school-taught material the normal adult retains with sufficient liveliness for it to be of much use to him. He took therefore groups of unemployed men and boys and groups of employed men and school pupils and employed various well-described methods to discover at least some of the answer to his problem. He found on the whole fairly marked gains in some subjects, especially native language, vocabulary, geography and a kind of general science; but tremendous losses in others, especially in arithmetic, algebra, geometry, chemistry and physics. He also found much disturbing evidence to the effect that many of the better grade pupils leave school early, leaving perhaps less naturally efficient ones to fill the higher school places. And he also discovered many interesting facts with regard to the selective and educative functions of certain occupations. The book could perhaps have been condensed, but it contains very good summaries to help the hurried reader. The research was well worth doing, and ought to stimulate further developments.

Guidance for the High School Pupil. By E. C. WEBSTER, with a Foreword by W. D. TAIT. Montreal: McGill University (London: Humphrey Milford). 1939. Pp. xvi + 153. 6*s*. 6*d*. net.

This volume is published in the McGill Social Research Series. After a general discussion of the main problems involved in fitting education and the student to make use of High School training, the author studies in detail: Who goes to the High School, what is his normal Progress through the Grades, and how important it is to make a thorough and controlled study of the Individual Student. He gathers together the results of his survey in a series of clearly stated conclusions, dealing both with modifications which he desires to see in the school system and with the development of methods for assisting the student. The monograph is well-planned and executed, and all those who are interested in practical, but liberal, education in this country have much to learn from it. The final conclusion is one of great interest in this country, for it reinforces what has often been said in England and is based upon a wide and well-attested mass of facts: "The recommendation of school leavers", says the author, "to particular occupational groups can be finally justified only if adequate employment opportunities are available. Without the central organization of placement services, little can be done to help the pupil who drops out of school before completion of his course."

It may be noted that the author pays a tribute to Dr C. S. Myers and the National Institute of Industrial Psychology, and to Dr Angus Macrae in particular, from whom and at which he received both part of his training and much of his inspiration.

Shell Shock in France, 1914–1918. By CHARLES S. MYERS. Cambridge University Press. 1940. Pp. xii + 146. 4*s*. 6*d*. net.

This is an account of some of the author's experiences from the outbreak of war in August 1914 to his demobilization in March 1919. It is fortunate, though it must have been exasperating at the time, that doctors over forty were not being accepted, in 1914, for service overseas; otherwise Dr Myers might have found himself entangled in the meshes of the army machine as a Regimental doctor instead of being able, as he was later on, to develop his own special line of interest. After various attempts to obtain medical work with the forces, Dr Myers was eventually appointed to the Hospital unit taken out to France by the Duchess of Westminster. In December 1914 he was gazetted a temporary Captain in the R.A.M.C. and in August 1916, by which time he had risen to be a Lieut.-Colonel, he was appointed Consulting Psychologist to the British armies in France.

Two distinct threads are interwoven in the narrative: (*a*) the recognition of, and the development of the methods of dealing with, the war neuroses; and (*b*) the heartbreaking delays and difficulties in getting anything done.

The historical account of the differentiation of the neurotic from the concussion cases has not, so far as the reviewer knows, been outlined so clearly and fully before and is well worth perusing; while there are two excellent chapters on the features, causation and treatment of 'shell shock' which will interest the general reader and be of value to the medical officer or practitioner who has no previous knowledge of the psycho-neuroses. Other chapters deal with the disposal of the cases in France and England.

The account of the author's dealings with the higher authorities makes mournful reading, especially in these days when the nation is fighting for its life. Fokker, in his autobiography, also complains of the difficulties he, fortunately for us, encountered from the higher command of Imperial Germany in getting improvements in his aeroplane designs accepted. One wonders if *both* sides in the present conflict are still floundering in the same quagmire.

Psychiatric Social Work. By L. M. French. New York: The Commonwealth Fund (London: Humphrey Milford, Oxford University Press). 1940. Pp. xvi+344. 13*s.* 6*d.* net.

This book describes in detail the history and present status, functions and training of psychiatric social workers in the U.S.A. Since the main emphasis is on actual practice, which appears to be extremely variable, it is not of great interest or importance to the English reader. It seems that the American psychiatric social worker may be allowed a considerable freedom in psychotherapeutic practice which the scope of her training and experience do not altogether justify. But there is an interesting discussion in Chap. vi, Some Trends in Social Treatment, of the transition which has occurred in recent years from the more 'active' type of treatment with its prolonged intellectual discussion of difficulties and its forceable manipulation of the environment, to the more 'passive' type in which some effort is made to help the patient and his relations to a real emotional understanding of their difficulties.

The Human Mind. By Murdo Mackenzie. London: J. and A. Churchill. 1941. Pp. vii+215. 7*s.* 6*d.* net.

The book is worth reading even though it seems to achieve formulation and a way of thinking rather than genuine psychological explanation. It begins with a somewhat sweeping statement: "In its early days Psychology was the study of man's behaviour and emphasized his sensations, emotions and instincts rather than his organ of thought. The notion of man's mind as a discrete organ with a specific function, working in rhythm in space and time and driven by strictly mental forces is, I think, new, and elaborated for the first time in this book." It is strange to set this view beside the psychologies of some twenty or thirty years ago which attacked all the earlier psychology for its over emphasis upon ratiocination and its belief in some unique thought function. To Dr Mackenzie the mind is an organ working in space and time, and following in space the principles of Simplification and Amplification, and in Time those of Immediacy and Deliberation. Into this framework he is able to fit all the principal facts which the psychological physician needs in his attempt to guide unhappy and maladjusted lives into more healthy ways. And it seems to be as good a framework, for all practical purposes, as many others. It has the great advantage, too, of not landing up the physician in deep technicalities and high-faluting phraseology. Dr Mackenzie is able to take instances of most of the better-known abnormalities and to apply to them the general categories of his scheme, and in doing this he has much to say that is well worth noting. He writes with sympathy and much understanding of many cases which have come under his own care. If the whole essay leans perhaps itself towards Simplification—in at least one of its senses—this is no great fault, and indeed is a relief in the general welter of a good deal of current psycho-pathology. When, however, he applies his principles to the social problems created by the modern Dictator and his vagaries, as he does in the final two chapters, the treatment bears signs of hasty preparation and insufficient care.

Psychology, Psychotherapy and Evangelicalism. By J. G. MACKENZIE. LONDON: George Allen and Unwin, Ltd. 1940. Pp. xiii+238. 10*s*. 6*d*. net.

The essay ranges over many fields in psychology, psychopathology, theology and philosophy. Prof. Mackenzie writes with great learning and consistent sincerity. He never shrinks from argument and is not infrequently sweeping and dogmatic. He believes that the psychology of religion must now proceed by the study of particular types of religious experience and he selects evangelicalism for his study because it is perhaps the most outstanding kind of religious bent that is directly based upon first hand experience. "What unites all Evangelicals, indeed enables them to be classed as one type of religious devotee, is an experience which they believe to be of God's grace mediated through an experience of Christ." Evangelicalism, like the psychoneuroses, is the outcome of conflict, perhaps sharp and dramatic, perhaps deep but not salient. But while the psychoneuroses try to avoid the issue, evangelicalism is said to face it and solve it. Hence much of the central part of the essay has to do with conversion. Prof. Mackenzie is convinced that while the solution to life's deepest conflicts lies in evangelical experience, anybody who wishes to help others to this solution will do it all the better with a knowledge of psychology.

Fear may be Liars. By JOHN RYLE. London: George Allen and Unwin, Ltd. 1941. Pp. 95. 3*s*. 6*d*. net.

No doubt there will be many who will be helped and enlightened by the kindly reasonableness of Prof. Ryle's approach to the age-old problems and terrors of death, pain and fear. But the psychologist knows that our feelings about these are inevitably confused by irrational phantasies, desires and actions originating in the unconscious mind, which a rational objectivity can do little to counter. Here lies the importance of the faiths which seem to Prof. Ryle so unnecessary. It is not justifiable for the biologist to dismiss them as futile without taking into consideration their relationship to the other phenomena of the unconscious mind.

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NATIONAL CHARACTER[1]

BY M. GINSBERG

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I. THE PROBLEM AND ITS DIFFICULTIES

IN popular thought and in serious historical and political studies the existence of national characteristics and even of national character, in the sense of coherent group of traits exhibiting some measure of permanence and continuity, is undoubtedly frequently taken for granted. An American statesman in speaking recently of the trend of the war predicted a German defeat by internal collapse, because as he said 'the Germans will not take it'. The novelist Conrad, when asked in 1914 what England would do, felt confident in asserting that if England did come into the war then, no matter who might want to make peace at the end of six months at the cost of right and justice, England would keep on fighting for years and, if necessary, alone. In both these instances it is clear that to Germany and England is attributed sufficient continuity of character to justify prediction of their probable behaviour in given circumstances. On the other hand, when we turn to the attempts that have been made to subject national characteristics to scientific analysis the results are generally so indefinite as to raise a doubt whether the characteristics in question exist at all. It is therefore of importance at the outset to bring out the difficulties which such studies of necessity encounter.

There is, to begin with, the obvious difficulty of avoiding personal bias in observation and interpretation. The greater part of the books on the subject are *livres de circonstance*, written under the influence of particular political situations and with a view to future policy. The numerous accounts of English national character written by Germans since the beginning of the nineteenth century may be cited in illustration.[2]

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[1] Inaugural address to the Social Psychology Section of the British Psychological Society delivered at Nottingham, 17 April 1941.

[2] An interesting discussion of these accounts will be found in (1), Introduction, pp. 35–51.

The earlier writers saw in the slow and organic growth of England and its sturdy independence a model for Germany's struggle and reconstruction. Later and under the influence of the new racial theories stress was laid on the ruling qualities of the Germanic peoples, and England is depicted as the noblest people of Germanic race. Later still, with the emergence of the theory of *Realpolitik*, there is a strong tendency to depict English character as essentially materialistic, as persistently striving for power and profit and hiding its selfish sentiments under a cloak of ethical and religious humanitarianism. This tendency was naturally encouraged during the war of 1914–18 and is clearly seen even in writers who claim, not without reason, to be capable of viewing the problem with some coolness and fairmindedness. Thus Levin Schucking seeks to show that hypocrisy is deeply rooted in English character. It is not, he thinks, to be explained as a defence mechanism against a too stringent Puritanism, because the trait is most marked in the non-Puritan sections of the English public and can be traced back to the pre-Puritan period (2). The best known work on England written after the war of 1914–18, while praising the sanity, virility and spiritual balance of the English, is yet at bottom dominated by the belief that the ruling quality of the English mind is the lust for power and that this constitutes a great danger to the world, unless it is checked and controlled by the resistance of something like an equal opponent ((3), p. 506). English and French writers in dealing with the psychology of peoples other than their own show, I think, greater powers of detachment, a keener awareness of the difficulties of entering sympathetically into the minds of others, and a greater readiness to make allowance for national and personal prejudices. To give one example, Fouillée's work (4) on the psychology of peoples is generously conceived and executed, inspired by the belief that the qualities of a people are as a rule more deeply hidden than its vices and absurdities, and that the lesson of comparative psychology is to inculcate at once justice and sympathy. His study of the character of the Germans, in particular, is extraordinarily fair-minded, though he cannot altogether avoid being disturbed by their growing militarism and chauvinism. A survey of the works dealing with national characteristics suggests, I think, that the errors due to bias can be to a large extent corrected by a comparison of the views that writers of different nations have of other nations than their own, while occasionally the picture they paint of other nations may also serve to reveal something of the mentality of their own. In this branch of social science as in others impartiality is very much a matter of degree, and in a measure the personal equation can be allowed for by paying attention to the historical conditions likely to affect the results of the enquiry.

Another set of difficulties arise from the great complexity of national groups and the fact that they differ widely in inner unity and homogeneity. There appears to be no generally accepted definition of nation, but for the present purpose we may say that a nation is a group of people, inhabiting or associated with a given territory, who regard themselves as a unity and have the will to give expression to that unity in political independence or at least in a measure of cultural autonomy. It is obvious that this definition would cover groups differing widely from one another in inner cohesion and complexity of structure. We cannot assume that all peoples who call themselves nations have a distinctive character, still less that this character is natural or native or equally diffused in all portions of the people.[1] A differential or comparative psychology of nations would have to establish a serviceable classification of types of nationhood. Such a classification would have to take into account (*a*) the type of political unity, e.g. whether federal or centralized, (*b*) degree of social differentiation and type of class structure, attention being given in particular to the emergence of the middle classes, (*c*) degree of cultural homogeneity, with special reference to language and religion, (*d*) age or rather stage of maturity or growth.

Politically complex nations often have sub-nations within them, as is, for example, the case in Great Britain, and the question arises whether there is a national character common to them all. Again, in most European countries there are important local differences both of temperament and character. The Picardian differs from the Gascon, the Norman from the Savoyard, and we cannot be sure that behind these divergencies there is an underlying identity of character.[2] The age of the nation or its degree of maturation becomes especially important in comparative studies. The traits attributed by Tacitus to the Germanic tribes have often been regarded as specifically German; but according to modern ethnologists these traits are common to many primitive peoples. There may be differences also in speed or rate of maturation, and it has been argued, for example, that the German people has matured very slowly and that it is only in recent times that its own nature has been able to express itself over against the numerous foreign influences to which it has been for long subjected (cf. (7), p. 39). The degree of social differentiation is especially important in dealing with the non-industrial peoples. Most of the characteristics attributed to the Poles, for example, appear to refer only to the Polish nobility, while of the bulk of the population we know little or nothing. In the more differentiated peoples it is perhaps an open question whether class characteristics are not at least as important as the national characteristics, and it is arguable that in some cases members of the upper classes have more in common with their opposite numbers in other nations than with the lower classes in their own. It is clear that a comparative differential psychology would have to take into account these considerations. Meanwhile, attention is most usefully confined to nations of long standing and in comparative studies to peoples of similar levels of cultural development. It must be confessed that most studies of national characteristics have so far been very general in nature and have hardly even begun to deal with the problems suggested by the above considerations.

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[1] Cf. Nietsche's remark: "That which is at present called a nation in Europe, and is really rather a *res facta* than *nata* (indeed, sometimes confusingly similar to a *res facta et picta*) is in every case something evolving, young, easily displaced" ((5), p. 208).

[2] An interesting attempt is made by Halbwachs, *Les causes du suicide*(6), to calculate the 'dispersion' of suicides in the various regions of several European countries. He shows that in this respect there is greater uniformity in England than in other countries, the tendency to suicide being more evenly distributed in the counties of England than in the departments and provinces of France, Italy and Germany.

There is still another sort of difficulty which arises out of the lack of a generally accepted theory of the structure of individual character. Fouillée's work is based on the old theory of the temperaments. Thus he regards the Spaniard as *bilieux-nerveux*, the German as *flegmatique*, the Frenchman as *sanguin-nerveux*, while he also finds important differences in sympathy, strength of will and in intelligence ((4), pp. 144, 254, 456). McDougall bases his account on assumed differences in the intensity of the basic instincts, e.g. gregariousness, assertiveness, submissiveness, and in temperamental traits like the tendency to introversion or extroversion (8, 9). Others work with the concept of types as when Madariaga regards the Englishman as the 'man of action', the Frenchman as the 'man of thought', and the Spaniard as the 'man of passion' (10). Whatever the starting-point of the enquiry, the problem is complicated further by our ignorance of the relative importance played by hereditary and environmental factors in the formation of character. Temperament is often regarded as the innate element in character and, in so far as national characteristics are held to be constant, this constancy is attributed to the persistence of temperamental traits. But there seems to be little agreement as to what is to be included under temperament, and the study of its hereditary basis appears to be still in its infancy. Again, it seems to me very doubtful whether differences in behaviour among different peoples can be safely attributed to differences in instinctive endowment. The French are held to be more sociable than the Germans, and this is explained by McDougall, for example, as based on a difference in the intensity of the gregarious instinct. But it may well be that the difference is rather in the way gregariousness or sociality expresses itself in the two nations. The French meet to talk, to exchange ideas; the Germans to drink, to make music, to go on excursions. Further, those who are not satisfied with enumerating single traits of different peoples and wish to show how these traits unite to form distinctive characters conceived as systems must find the problem of the relative importance of hereditary endowment and environmental influences still more difficult, since very little appears to be known with regard to the genetic basis of the organizing principles of character and personality. In view of all these uncertainties in the theory of individual character, it is not surprising that the psychology of peoples has found no secure foundation and that the bulk of it consists of divination rather than observation, intuitive impressions rather than scientific analysis and interpretation. Despite all these difficulties and obscurities it seems to me to be a mistake to dismiss the whole theory of national character as an 'illusion' (11). The dangers arising out of bias and the intrusion of value judgements are not peculiar to this branch of study but beset all social studies in varying degree and can be to a great extent watched and guarded against. The difficulties of comparative study can only be removed gradually as our knowledge of the different nations grows and is supplemented by more detailed study of the character of groups within the nations. The problem of the genetic elements in national character has been, as we shall see later, much obscured by the assumption that national characteristics are racially determined. This has led to much futile argument. In all probability it would be much better to defer discussion of the part played by genetic factors until a better technique of record and observation has been developed and the relations between individual and social psychology have come to be more clearly defined.

II. THE DEFINITION OF NATIONAL CHARACTER

I propose now to enquire what exactly is understood by national character. Strictly speaking, I suppose, only persons can be said to have character and, if we follow Stout in saying that "Character exists only in so far as unity and continuity of conscious life exists and manifests itself in systematic consistency of conduct" ((12), p. 653), then nations cannot be said to possess character. Nevertheless, nations like other groups behave in distinctive ways and if their behaviour shows some unity and continuity we may perhaps speak of a group character without committing ourselves to any theory of a group mind or group personality. In practice there seem to be two senses in which the term is used. It may, first, be used to indicate differences in the distribution of certain traits or perhaps of types in different groups, as when we say that the Germans are more docile than the English or that the French are on the whole more articulate than the English, or that the Irish temperamentally dislike regimentation. This could perhaps be called common character, and it is clear that a scientific study of it would involve a statistical examination of the frequency with which traits are distributed in different groups. But, secondly, group character is also used to indicate the behaviour patterns of the group as a whole, that is the nature of its organization as embodied in its institutions, its collective achievements and its public policy. It is then assumed that the institutions in the long run depend upon the character of the component individuals as shaped by their interactions. Clearly there is a very subtle and complex relation between institutions and the character of the individuals sustaining them, since on the one hand men make their institutions but, on the other, the institutions make the men. Hence it seems misleading to say, for example, with Madariaga that it is not English education which explains the Englishman, but the Englishman English education, or that the Reformation and the Roman Catholic Church are not to be considered as causes but as effects of national character. The relation is one of reciprocal interaction rather than of causality. The situation is complicated by the fact that institutions may not reflect the character of all the members but perhaps only of powerful sections, and that, once formed, they tend by unconscious processes to select the type that suits them. In this way many qualities in a population may remain dormant or repressed until a change of circumstances brings them into play, while again the reaction against a type that has become unduly dominant may sometimes be an important cause of change. It is thus possible for great and revolutionary changes to take place in the institutions of a nation without a parallel change in the underlying qualities of the mass of the nation. It follows that at any one moment of time we cannot safely infer the character of a people from its institutions or public policy, and that for this purpose it is necessary to know the history of the institutions and the portions of the people that have been dominant in shaping them. A great deal of what foreign writers have to say about

English character is based on their impression of public school mentality. But the public school tradition dates only from the middle of the nineteenth century and few of the 'Prep' schools which are an integral part of the system date further back than 1870. In dealing with the psychological basis of religion in England the class structure of the country becomes of great importance, since it is generally agreed that, on the whole, Anglicanism has been the religion of the gentry and of the villages, while non-conformity is more characteristic of the middle classes and the towns. The range of variation in character has to be taken into account. Most continental observers find English character homogeneous throughout the different classes and they attribute this to the lesser rigidity of the class structure of England as compared with, say, that of Germany. But this is hardly more than a subjective impression and more accurate methods of observation and record would no doubt reveal both local and class differences. The transition from the institutions of a people to its underlying character thus raises many difficult problems.

Similar difficulties arise when the literature, the science and philosophy of a people are taken as a basis for the psychology of peoples. Fouillée thought ((4), p. 547) that a nation expresses itself above all in its *élite*, its representative men: "If you leave out of consideration the élite of France there is no more France; France is reduced to the level of those peoples who have no history." "There is no national poet of any great nation", it has been said, "who is so completely representative of his own people as Shakespeare is representative of the English", and it has even been argued that his acceptance by the world is due not so much to the greatness of his genius as to its English quality ((13), chap. VI). On the other hand, of Dürer and Bach a German writer has said that they are representative rather of Protestantism than of Germany (14). It is difficult to see how at the present stage of our knowledge such problems can be resolved. In any case, the study of national traits as revealed in its 'representative' men must be supplemented by a study of the mentality of the people at large, especially as revealed in proverbs, folk lore and especially in wit and humour.[1] There is here clearly an enormous field of study.

The bulk of the more popular works on the psychology of peoples is concerned with national character in the first of the two senses distinguished above, namely the wide prevalence of certain traits in the group.

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[1] Cf. Baldwin's remark: "Understand English humour, and you have gone a long way to understanding the Englishman" ((15), p. 22).

They abound in rough generalizations and often exaggerate both the permanence of the characteristics and their wide diffusion. The English have been described since Froissart as 'taking their pleasures sadly' and their gravity has been contrasted with the gaiety of the French. But already Emerson noted that their 'brows were no sadder than their neighbours of northern climates', and as compared with Americans he thought them cheerful and contented. The experience of the war of 1914–18 belied the traditional opinions about the psychology of peoples. Clemenceau remarks: "The Englishman was noted for his calm, but English soldiers tended to be more hysterical than others, the Americans were supposed to be so quick and they were so slow. The French were supposed to be gay and they were solemn" (quoted in (11), p. 39). The Chinese are reputed to be impassive and reserved in the expression of their emotions, and even a careful anthropologist like the late Professor Seligman felt justified in describing them as introvert. He found, however, on closer enquiry that what he took to be an inborn trait of the Chinese was rather a mode of behaviour set up as a standard for the governing and literary classes and never adopted by more than a small proportion of the population ((16), p. 86). So again the tradition of the Austrian as charming, easygoing and kindly was shaken by the brutalities committed after the triumph of the Nazis. Examples could easily be multiplied of similarly crude generalizations and especially of the danger of predicting the future behaviour of peoples on the basis of what is supposed to be their permanent and even innate character. Yet even when allowance is made for local and class differences and for the effects of changing circumstances there is often some truth in the generalizations reached by careful observers, and it would, I think, be a mistake to deny the existence of national traits in the case of nations of long standing merely on the ground that so far they have not lent themselves to exact analysis or quantitative measurement. Better results may be expected when more is known of the genetics of character and when a more reliable technique has been elaborated for observing and recording group differences in behaviour.

The indirect method of studying national characteristics by an analysis of the psychological basis of the collective achievements of peoples has undoubtedly proved more fruitful than the direct method based on the observation of individual behaviour. As an example I want to consider briefly some of the views that have been put forward regarding the character of the English and the Germans. With regard to two qualities of the English mentality there is universal consensus among observers both English and foreign, namely, its empiricism and individualism. Apart from the contributions made by English thinkers to empiricism regarded as a philosophical theory, the empirical habit of mind is seen in all spheres of English life. English law and English politics are based on the empirical method of dealing with particular problems as and when they arise. There is a disinclination to formulate general principles and piece-meal enactments are preferred. Even when, as has frequently been the case, legislation is undertaken after special enquiry, the enquiry is deliberately limited in scope and little attention is given to the requirements of general theory or systematic connexion.[1]

English international policy is especially characterized by its tentative, piece-meal method. It avoids legal rigidity and has no faith in general solution or long-range planning. An acute French observer has seen in this trait one of the standing difficulties of Franco-British collaboration. In contrast with the habit of the English diplomatists to confine themselves to the immediate and particular, the French mind reaches out for the general and has a longing for clear-cut and logically consistent solutions. That there is a certain consistency in British foreign policy over long periods of time is due in the main to the geographical and economic situation of Britain and certainly not to the existence of deep-laid plans pursued year in and year out with Machiavellian cunning. If the British Empire was not exactly built up in a fit of absence of mind neither was it the result of a deliberate policy carefully planned beforehand and applied methodically by successive governments. The early French colonial acquisitions were also fortuitous and occasional and not the results of conscious planning. But this cannot be said of the colonial policy of the Republic which appears to have been due to a political plan carefully studied and applied with persistence in the face of great difficulties (cf. (18) and (19), p. 25). German colonial policy was, of course, still more consciously and deliberately planned, with frank recognition of its economic nature and with the open support of financial and commercial interests. In internal policy the same characteristics of the English mind can easily be traced. The principles of democracy have never been clearly formulated and deliberately applied. The issues fought over have been particular changes in which only the historian can discern a steady tendency. The history of the Church of England also shows a quality essentially connected with empiricism, namely a dislike of pushing principles to their logical conclusions in face of the complexities of life. The dislike of the English for rigid principles and their distrust of abstractions is sometimes attributed by continental writers to a lack or weakness in the power of generalized thought. This seems absurd in view of the contribution of the English to science and philosophy. But it is true that in dealing with the practical problems of life the English mind prefers to proceed tentatively, by trial and error, and in this it appears to me to show a better and sounder sense of method than those who put their trust in dialectics.[1]

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[1] For numerous illustrations compare especially Boutmy (17), chap. IV.

The second quality of the English mind, its individualism, can also be readily traced in the various spheres of the national life. It can be seen in the spirit of the English law which is a law of the liberty of the individual subject, in the strength of local government and resistance to centralization, in the stress laid by Puritanism on the autonomy of the individual and in a very widespread and deeply rooted impatience of compulsion and restraint. Continental writers, while stressing the individualistic tendencies of English institutions, have nevertheless also urged that this individualism has its limitations. Both Madariaga and Dibelius think that the tendency of English education and especially of the Public Schools and the Universities has been to standardize behaviour and to repress the types that threaten to diverge from the norm. A recent writer, very friendly to England, has said that as compared with pre-Hitler Germany "the English type in all classes lacks a certain touch of individualization" (21). I suspect, however, that these writers are impressed rather too much by a certain conventionality in the behaviour of some of the educated classes in England, which on closer analysis proves to be perfectly compatible with a plentiful variety and even eccentricity of character.

The individualism of the English is closely associated with a capacity for spontaneous organization, and indeed a great deal that is distinctive in English society rests on the power of combining an intense individualism with practical understanding of the needs of others. That these qualities are not peculiar to any particular social class in England can be seen from the enormous number of voluntary societies in England and, in particular, from the history of the trade unions, the cooperative movement and the Friendly Societies. I think Madariaga has laid his finger on a very important point when he stresses as a remarkable and typically English phenomenon which he calls "collaboration in opposition" ((10), p. 22) and which he shows to be as important in the field of politics as in sport. With these qualities are associated also the capacity for collective deliberation, the toleration of divergent views and the considerateness shown to opponents, which are characteristic of so much of the social life of England. Social discipline is proved not to be incompatible with a respect for individual pecularities and with freedom to criticize accepted and sanctioned views. The prestige of the law, for example, is not shaken in the least by the perpetual stream of satirical criticism directed against it. A French observer has said: "There is no country where the judges, i.e. the persons whose duty it is to administer the law, criticize it with more irony, or ridicule it with more humour, and that from the Bench, in the presence of the litigants" ((17), p. 177). Equally illuminating is a comparison of the habits of debate or public discussion in England and other countries. I doubt whether any country excels England in the fundamental decency of public discussion, the urbanity and moderation which is shown to opponents and the care which is taken to keep out 'personalities' or imputation of bad motives to those from whom one happens to differ.[1] It remains to be added that these qualities which I have stressed as exhibited in the public life of England are not independent but closely interwoven. The empirical habit of mind harmonizes well with the attitude of tolerance and respect for other persons which is implicit in individualism. It is in the fusion of these qualities with the tendency to spontaneous association which gives distinctive character to English institutions.

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[1] Cf. the remarks of T. S. Eliot ((20), p. 352): "The admission of inconsistencies, sometimes ridiculed as indifference to logic and coherence, of which the English mind is often accused, may be largely the admission of inconsistencies inherent in life itself and of the impossibility of overcoming them by the imposition of a uniformity greater than life will bear." Cf. also the rather flattering account given by Madariaga ((10), p. 63).

Strangely enough, individualism is a quality which authorities both German and non-German have taken to be deeply rooted in the German character. A distinguished student of comparative religion, Otto Pfleiderer, finds in the history of German Protestantism ample proof of an extreme individualism, of a tendency of the personality to isolate itself at once from the outer world and the community and to seek in its own being the connexion with the divine (22). The older descriptions of social life in

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[1] Of German debates Muller-Freienfels writes: "In hardly any other country of the world do political opponents so regard each other as moral scoundrels as in Germany, hardly anywhere else is political opposition so saturated with hatred as among ourselves.... Just as two German scholars who differ in their opinion regarding the value of the Niebelungen manuscripts invariably despise one another morally, so it is in the field of politics" ((7), p. 144). "In France", says Madariaga ((10), p. 158), "a debate is a battle and arguments are loaded. Every pair of eyes looks like the two barrels of a double-barrelled machine gun turning at high speed, throwing ideas at the enemy with the utmost alacrity. Arguments, innuendoes, accusations, insults are hurled through the air like projectiles."

Germany give a picture of people highly individual in their taste, unconventional, deliberately odd in their behaviour, blunt and rather brusque in their personal relations, stiff and reserved. In their political life this individualism is seen in a strong tendency to particularism and discord and an incapacity for wider unions except when they come under the influence of dominant leaders. Every German, said Bismarck, would rather have his own king. Their parliamentary history proves, as a recent commentator has pointed out, that every German would also like to have his own party. Their lack of political capacity seems to be the trait that has struck most observers. The individualism of the Germans thus differs from that of the English in not being accompanied or balanced by the capacity for spontaneous organization but requiring evidently organization based rather on some form of subordination.

Another quality generally ascribed to the Germans is strength of will. This is seen in economic life in their industry and in scientific work in the persistent 'laboriositas' already noted by Leibniz. There seem, however, to be certain characteristics of their thought and feeling which colour alike their will and their individualism. On the cognitive side the German mind seems often lacking in concreteness. Though the German works with abstractions, these do not seem to be reached by analysis of sense experience but rather by a sweep of imagination or fantasy. A German writer has characterized this tendency in the words: "Die Eigenheit der deutschen Abstraktheit ist Begriffsphantastik" ((7), p. 88). Their intellect is not inspired by a search for order and clarity. Their interest in system again is often not rooted in the need for order, not the product of a drive to classify and understand, but rather of an imaginative longing for grandiose architectural schemes. Hence an *esprit large et trouble* which with a certain profundity and comprehensiveness often combines a tendency to vagueness and obscurity. On the side of feeling the German writers speak of a depth and inner warmth (*Innigkeit, Wärme, Tiefe*), but they also point to a vagueness and indefiniteness which the non-German describes as sentimentality. This combination of cognitive and affective vagueness shapes the nature of the will which, though thorough and realistic in many ways, is also liable to be moved by vague and highly abstract ends. What the German calls his idealism, says Muller-Freienfels, is a devotion to fanciful abstractions. It is rooted in feeling and fantasy and tends to evade and even resist rational control or definition. No doubt this can be said also of other peoples, but it does seem to be true that Germans are more apt to be moved by large but vague ends, that their will, like their thought, is lacking in the sense of proportion or measure which is found in the English and the French.[1]

This account of the German as highly individualist and as led by vague and fanciful ideals, an account which has the authority not only of German writers but also of foreign observers, has of course to be brought into relation with another side of German life, with which at first it seems to stand in glaring contradiction. What of what is called German militarism, German docility, German thoroughness and attention to detail and method? Muller-Freienfels solves this problem by resorting to the mechanism of compensation and over-compensation. Afraid of the anarchy which his deep-rooted individualism would produce, the German is ready to accept the leadership of a strong personality; fearful of the conflicts which his powerful but indefinite strivings must generate, he has recourse to minute regulation and prescription of every detail of group life; aware of the dangers of his speculative fancy, he insists on exact method and painstaking investigation; to keep his feelings in check he cultivates hardness and reserve. These are all compensations, it is claimed, developed by the Germans in the course of a century-long struggle with their own nature, especially under the discipline inculcated under Prussian influence. The process of compensation on this view of the matter is largely unconscious and is by the Germans themselves regarded not as based on reasoned calculation of consequences but as irrational or suprarational. Hence what other people call militarism is not thought of as such by the Germans and what strikes others as slavish submissiveness is by them thought to be willing acceptance of constraint.

Muller-Freienfels appears to think that the traits of docility, thoroughness and discipline are secondary and not primary features of German character. It has, however, to be remembered that the notion of compensation is easy to invoke but hard to verify, and in any case, if the compensation is genuine it is likely to be deeply rooted in unconscious need, and consequently to be an integral element of character. Other writers give a different and perhaps more plausible account of the duality of the German character. Lacking in the power of spontaneous organization, which in the case of the English provides a balance to the forces of individualism, the Germans have only been able to achieve such unity as they have by authoritarian discipline. Realizing this need for regimentation they have consciously and unconsciously divided their lives into separate zones, a private zone of freedom and independence and a public zone of deliberate acceptance of constraint. Writing in 1893 Professor Richard M. Meyer tells us: "The German frequently sadly disregarded political liberty, because he knew his liberty of thought to be safe from attack" ((14), p. 241). Dr A. Loewe tells us in a recent work that "up to the present the German Philistine, even when most impressed by political authority, has always managed to maintain his personality in his own eyes. By a private interpretation of the universe he tried to give to the most banal life the consecration of supra-temporal freedom" ((21), p. 25). Now that the Totalitarian regime has invaded even the personal zone of life it remains to be seen whether the individualist tendencies will have sufficient strength to assert themselves. It is clear that, for whatever reasons, the need for authority is deeply rooted in German life and that the relationship of inferior and superior pervades all spheres of activity. A prolonged period of education in other forms of organization will therefore be necessary before Germans are led to abandon the forms of order resting on authority and hierarchical subordination.[1] The view is often advanced that towards the end of the last century and the beginning of this century the Germans underwent a radical change of character which turned them from a nation of thinkers and dreamers into a nation of cool and hard realists. It seems more probable that with certain changes of aims the fundamental characteristics have remained unaltered. Especially noteworthy is the persistence of vague and cloudy aspirations and the admiration for the demonic, the heroic and the like. German *Realpolitik* and *Machtpolitik* are in many ways realistic and pursue perfectly definite and concrete aims, but equally the appeal which they make is essentially romantic. The same combination of definite aims with appeal to myth and fantasy is to be observed in much of the propaganda of the Nazis.

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[1] Cf. Nietsche's remark: "As everything loves its symbol, so the German loves the clouds and all that is obscure, evolving, crepuscular, damp and shrouded; it seems to him that everything uncertain, undeveloped and growing is 'deep'" ((5), p. 198). Contrast Chateaubriand(23): "Le ténébreux, l'embroussaillé, le vapoureux, le pénible me sont abominables."

III. THE FORMATION OF NATIONAL CHARACTER

Having given some examples of the character of nations as reflected in their collective life, I propose now to say something on the factors which contribute to its formation. This question has undoubtedly been greatly obscured by being entangled with the problem of race differences. I have discussed the relation between nation and race elsewhere (24, 25, 26), and here I desire to draw attention to some of the more important points.

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[1] This at any rate was the view of many German writers before the advent of Nazism. Cf. Muller-Freienfels ((7), p. 159).

(*a*) The mental characteristics of the racial elements which have entered into the composition of the European elements are entirely unknown, and to explain national traits by reference to them is to explain the obscure by the more obscure. It is to be noticed that in recent anthropological work it has been found necessary to break up the three principal units, Nordic, Alpine and Mediterranean, into numerous subdivisions. Thus, for example, v. Eickstedt has three subdivisions of the Nordic (27), Coon four types (28). The Alpine and Mediterranean groups are not in any sense unitary entities. The psychological descriptions that are given of these racial groups still lump them together, but it seems extremely unlikely that what is said of the mentality of any of these large aggregates can really apply to all its constituent groups.

(*b*) The theory that the existing distribution of racial traits is to be explained in terms of hypothetical pure races supposed to have intermingled in various proportions is coming to be abandoned by modern anthropologists.[1] If such pure races ever existed we certainly know nothing of their mental characters. The descriptions we have of the habits of Gauls and Teutons cannot be taken as descriptions of Alpines and Nordics, since by all accounts they were racially composite and the part played by race in the determination of *their* mental characters is just as complicated and insoluble as the part played by race in the formation of the character of the French and the Germans.

(*c*) There is a difficulty of a more general kind. It is the habit of those who explain national character in terms of race to refer highly complex institutions to specific innate tendencies. McDougall ((8), p. 223), for example, explains the prevalence of a centralized system of government in France mainly by the supposed intensity of the gregarious instinct in the Mediterranean and Alpine races. But from gregariousness to centralized government is surely a far cry. The evidence of all those who know the French character intimately is to suggest that the attitude of the French to the state is not that which would be expected from a spontaneous play of the social tendencies. Madariaga describes the state in France as something mechanical rather than organic. M. Paul Gaultier stresses the fundamental individualism of the French, their lack of discipline and social cohesion, and he even speaks of an instinctive opposition to the state ((30), p. 156). It is clear that if social institutions are to be referred to underlying psychological causes, use will have to be made of much more complex constellations than the instincts, and it seems highly probable that from very similar instinctive equipment very different forms of social grouping may arise.

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[1] Cf. especially Morant (29).

(*d*) Any theory of national character must of course be able to account not only for the relatively permanent traits, but also for the changes that undoubtedly occur. On the racial hypothesis such changes are to be explained as due to various forms of racial changes brought about by emigration, immigration, conquest and social selection inside communities. An examination of the problems thus raised would require a lengthy treatise, but it may be worth while to give some brief illustration of the fundamental issues. As an example of explanation in terms of racial change due to war and immigration I may quote McDougall's assertion that the decline of the Greeks from the heights reached by them in the time of Pericles reflected a change in their mental qualities due to racial substitution brought about by wars, the slave trade and later by successive invasions of Slavs from the north. He goes so far as to commit himself to the statement that the modern Greek people is "descended largely from Slav invaders and largely from the numerous and prolific slave population of the great age of Greece, but hardly at all from the men who made the greatness of that age" ((8), p. 249). Such explanations seem to suffer from two main defects, namely, the lack of adequate anthropological evidence and a failure to attend to negative instances. In the case of the Greeks it is to be remarked that McDougall's categorical assertions are not borne out by recent anthropological studies. Statements about the physical character of 'the Greeks' in general are of little value. If comparison is to be made with the Greeks of antiquity, care must be obviously taken that the reference is to the inhabitants of the same localities. It appears from recent investigations that when this is done the continuity of living Greeks with their ancestors of the ancient world is more striking than the opposite. Professor Coon ((28), p. 604) tells us that "If one refers to the inhabitants of Attica during the sixth century, or to the Spartans of Leonidas, then the changes in these localities have probably not been nearly as great as that between the Germans of Tacitus and the living South Germans." The explanation of cultural changes in terms of racial substitution thus rests in this case at any rate on slender foundations. Even more serious is the failure of such explanations to take into account negative instances, i.e. instances of significant cultural changes not accompanied by known racial changes. A case in point is that of the Romans. The periods of vigour from the fourth to the last century B.C., and the later brilliance of the Italians during the period from the eleventh to the sixteenth centuries A.D., have frequently been ascribed to the infusion of fresh blood. This explanation, however, fails completely to account for the later decline in the sixteenth century and the revival in the nineteenth century.[1] In the case of the Risorgimento the explanation is to be found in political factors connected with the re-establishment of contacts with Western civilization resulting from the temporary incorporation into the Napoleonic empire. But, if so, may not the earlier successes also have been largely shaped by social and political factors?

The problem of the effects of race mixture presents great difficulties. It has been claimed on biological grounds that the blending of allied races results in an increase of constitutional vigour and in a greater variety and variability of innate qualities. This may well be true, but the importance of these changes for cultural change is not forthwith to be assumed. We have to distinguish between the effects of race mixture on the genetic constitution of the resulting blend of peoples, and the effects of cultural stimulus and variability. The forces at work cannot be clearly isolated, yet the numerous cases of important cultural changes due to culture contact unaccompanied by race mixture on a big scale should make us hesitate to stress the purely genetic changes. No one, for example, would question the enormous influence exerted upon the political structure of England by the Norman conquest, yet the invasion did not in any way modify the physical type of the English ((32), p. 82). Similarly, while the cultural effect of the Roman conquest on France was of decisive importance, the racial effect was negligible ((33), p. 157). An examination of the results of the contacts between European and non-European peoples is even more illuminating, since it at once becomes clear that the cultural effects are out of all proportion to the degree of racial admixture. It seems therefore highly probable that even in those cases where culture contact is accompanied by racial amalgamation the purely genetic effects of the mixture are not so important for the formation of national characteristics as the sociological effects of the stimulus due to contact with fresh elements of culture.

There remains the problem of the effects of social selection. By this is meant the influence exerted upon the genetic constitution of a people by the action of social forces. Thus for example long-continued differences in fertility between social groups may, if the groups differ in genetic qualities, result in a change in the character of the people as a whole due to the increasing recruitment of its population from the more fertile groups. Elaborate investigations have been conducted on these lines by the Anthropo-sociologists (Ammon (34), de Lapouge (35) and others). They claim to have shown among other things that Europe has undergone a process of 'denordicization' (*Entnordung*). This is supposed to have occurred through military selection (the Nordics being fighters and therefore tending to be killed off); through religious persecution (e.g. the emigration of Protestants); through urban selection (the Nordics being restless and enterprising and therefore attracted to the towns where they fail to reproduce themselves). I do not propose to discuss this particular view here. The evidence is considered by many competent authorities as insufficient to justify the formulation of general laws. It seems that there really has occurred an increase in brachycephaly, but the causes of this increase are disputed and some regard it as at least in part an environmental effect analogous to and accompanying the parallel increase in stature ((33), p. 278, (28), pp. 10–11). In any case so far no one has succeeded in establishing any definite relation between broad-headedness and any mental quality, and the psychological effects of the increase in broad-headedness remains a matter of speculation. Another case frequently cited in this connexion is that of Spain, whose decline is attributed to negative selection resulting on the one hand from religious persecution, which killed off or drove out of the country individuals of independent mind and will, and, on the other, from the excessive multiplication of monastic orders which made celibates of large numbers of people presumably of superior ability. This argument suggested by Galton ((36), p. 344) was elaborated more fully by Fouillée ((4), pp. 158 seq.). The biological effects of clerical celibacy must be a matter of doubt, since many of the priests were only celibates in name and since there is no proof that the majority of the priests differed from the rest of the population in innate independence or ability. Fouillée himself doubts whether the biological effects of celibacy, even if combined with the graver influence of the persecutions and expulsions, could have been such as to produce a really vital change in the quality of the population. He lays greater stress on the wars of Charles V and particularly the American conquests which drained Spain of men endowed with energy and the spirit of enterprise, and he explains that to these biological factors must be added economic and moral causes which he discusses at length. Apart from these rather speculative arguments there is a certain amount of direct evidence of the selective action of immigration. It has been shown for example that the Poles who came to the

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[1] Cf. the detailed examination of the facts by Professor Toynbee ((31), pp. 16 seq.).

United States during the nineteenth century and the beginning of the twentieth century differed in height, pigmentation and head-form from their own relatives who remained at home ((28), p. 8), and that they did not represent a cross section of the entire Polish population. It does not follow, of course, that this selective immigration seriously altered the innate constitution of the Poles who remained in Poland.

It has also been alleged that the wholesale emigration from Ireland and Sweden to America has permanently affected the genetic constitution of these countries, but, so far as I know, there is no direct evidence of this change. The effects of urban selection have been studied in many countries since the question was raised by the Anthropo-sociologists, without however any stress on the racial aspect of the process.[1] Though none of these investigations are conclusive, it is highly probable that over long periods of time social forces may act selectively on the genetic constitution of a people, and conversely the genetic constitution may in turn influence the forms of social life. Yet even a little reflection is sufficient to show that the processes involved in social change differ radically from those involved in racial change and that vast changes can be brought about in society without parallel change in the inherited constitution.[2] To begin with what is acquired by one generation can be transmitted to the next without resort to the genetic mechanism of inheritance. This makes possible cumulative change on a vast scale. Further, there is not the same need for continuity as in genetic transmission, since even ideas which have been forgotten may again be revived after a long interval of time and exercise renewed influence. Furthermore, co-operative division of labour renders it possible for societies to achieve results amounting to specific changes without any change whatever in the genetic constitution of the component members. For these and many other reasons racial change and social change appear to differ radically in their nature, and it is not to be expected that they should follow parallel courses.

These considerations have an important bearing on the problem of social selection. A sort of selection is operative in society which has nothing to do with the substitution of one genetic type for another. This may be shown by a few examples. It has been argued by Pirenne(38) that each of the phases in the growth of capitalism in Western Europe was initiated by 'new men' who made their way by exceptional audacity and vigour. Is there any reason to believe that these men were 'mutations' suddenly called into being by the circumstances of the time? It is surely far more likely that these qualities were always present in the population and that sections of it which hitherto were denied opportunity were able to seize it by a change in the social conditions affecting commerce. Similarly present-day Germany is ruled by a group of people of distinctive mentality, but they are hardly likely to be a fresh biological product. In different circumstances quite a different group may attain predominance and thus present to the world quite a different aspect of the German character. In short, the selection exercised by social forces need not operate by way of biological elimination or substitution. It operates rather by encouraging or inhibiting the expression of certain qualities and by determining their direction or mode of manifestation. A totalitarian regime may drive individuals of a certain endowment out of the country, and this may have some effect on the biological constitution of the stock. But in the main the hereditary qualities of peoples may well remain constant while changing circumstances bring different sides of the underlying character into play or give dominance to different elements of the population at different times. The process of adaptation is thus social rather than racial and, if the term 'selection' is to be used at all in this context, it must be given a meaning very different from that which it has in biology.

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[1] Cf. Coon(28), p. 8 for references.

[2] This was clearly pointed out by Huxley(37).

In summing up these arguments we must, I think, distinguish between genetic qualities in general and those which are claimed to be racial traits. Since at present very little is known of the mental characteristics of races, we can obviously make very little use of them in accounting for national characteristics. But this is not to say that genetic qualities do not count. Unless we are prepared to deny the inheritance of mental characteristics we must regard it as highly probable that just as there are individual differences there are also group differences and that these play their part in shaping the collective life of groups. The difficulty is to determine exactly what their role is. The inherited constitution must in some sense put a limit to what can be achieved by social organization. If the latter puts too high a demand on the character of individuals, it will not work, and, if a movement is forced by enthusiasts on a reluctant mass, it may come up against ineluctable predispositions which will in the end defeat it. Yet here again it must be remembered that the range of human potentialities is extraordinarily wide and that upon the same hereditary elements very different social structures may be built. There seems no warrant for assuming any such differences between national groups as would amount to an inherited incapacity of anyone for the arts and institutions achieved by another. We cannot infer from the incapacity hitherto displayed by any people to run democratic institutions that they must forever remain so incapable. If then there is an inherited element in the character of nations of long standing they nevertheless retain considerable powers of adaptation and the limits of these powers cannot be determined with any accuracy from their previous history.

In view of the above considerations it would seem that the differences in the character of the European peoples are not in the main to be traced to variations in innate tendencies, but rather to variations in the ways in which these are expressed, balanced and directed. Even in referring to highly complex qualities of character like 'individualism', 'empiricism', 'collaboration in opposition', we cannot say that these qualities are either present or absent in a given people. There is, for example, a French individualism, an English individualism, and a German individualism. So again different peoples may successfully work the same institutions—but with a difference. To account for these differences we have to look to historical and social conditions and only in the last resort should we appeal to genetic variations. If for example we seek to ascertain the conditions which have given a definite tinge to the behaviour of British diplomacy, we have no need to appeal to unverifiable genetic factors. Far more illuminating is the attempt made by such writers as Bardoux (39) and Kantorowicz (1) to analyse the historical conditions in which that behaviour has taken shape, e.g. the geographical conditions which have given to British diplomacy a certain constancy of aim, the peculiarities of the English class structure which has put the conduct of foreign affairs in the hands of a class with special codes of behaviour, the feeling of security and confidence generated by the absence of the fear of war and fear of revolution which has formed the background of continental diplomacy. So again the peculiarities of the political individualism of the English are not traced with any probability to the supposed inborn qualities of the Saxon peasant. They are surely connected rather with the special conditions which prevented the growth of a centralized administration and encouraged the growth of local government, and of a legal system serving to protect the liberties of the individual.[1] In a certain sense no doubt the institutions of a people express in the long run the character of the people, but equally the character is formed by the institutions. The congruence of the different aspects of a nation's life in particular depends on the unity and continuity of its historical development. It has been remarked, for example, that in this respect there is a profound difference between England and Germany. In England there is a certain homogeneity of texture in the various manifestations of the national life, while in Germany religion, government and philosophy seem rather to diverge in their development. The explanation is surely that in England the early attainment of political unity facilitated the interaction of the various elements of the national life, while in Germany the conditions favoured the perpetuation not only of marked local variations but of more rigid distinctions between professions and social classes.[1]

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[1] In a recent work (40), Profs. Buckland and McNair show the remarkable similarities that exist between the practical rules of the Roman Law and the Common Law, despite the difference in the fundamental conceptions on which the two systems rest. They attribute the similarity to a certain resemblance in the habits, the morale, the *Anschauungen* of the two nations and the difference in the conceptions to a difference in racial origin. It is difficult to see how such a hypothesis could ever be put to the test.

I will now try to sum up briefly the general conclusions which emerge from the above survey. First, the study of national character is to be approached not through an investigation of individual differences in behaviour, but of the qualities manifested in the collective life of nations, their traditions and public policy. Secondly, the object of this study is to discover whether these manifestations of the collective life reveal the existence of relatively permanent and stable traits and dispositions and how far these form congruent systems. Thirdly, such studies as have so far been made indicate that although ultimately national character must be linked with the genetic qualities of the stock, these are highly plastic and susceptible of wide variation in their actual expression. Hence the national character is not something given once and for all, but something always in the making, moulding and being moulded by the circumstances in which nations find themselves.

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PERCEPTION DURING DARK ADAPTATION

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From the Cambridge Psychological Laboratory

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I. Introduction

It is a well-known fact that individuals differ in their ability to 'see in the dark', a fact that becomes of great importance during the 'black-out'. It has generally been assumed that differences in this ability were dependent on differences in the absolute threshold of vision, and its modification during dark adaptation. But 'ability to see in the dark' means in effect 'ability to perceive in the dark'; and it has never been shown that the variation of perceptual ability during dark adaptation is a function of the variation in the absolute threshold and of this alone. In fact, it seems probable that other factors are operative in determining perceptual ability during dark adaptation. And therefore it is a matter of some interest and importance to discover the nature of these factors.

In consequence, series of experiments were designed to measure, first, the changes in the absolute threshold of vision for various individuals during a fairly long period of dark adaptation; and, secondly, the changes in perceptual ability of the same individuals during a corresponding period. Now it seemed probable that 'perceptual ability' is not one indivisible entity functioning regardless of the nature of the material to be perceived, or of the situation in which it is perceived. Consequently the following varieties of perceptual material and situation were employed, in the hope that they might afford some idea of the possible variations of perceptual ability according to material and situation:

(1) A very simple perceptual task: The subject was required to report the orientation of a white dial hand on a black background.

(2) More complicated perceptual material, allowing much greater freedom of interpretation: The subject was presented with a series of meaningful silhouette pictures, black on a white ground, and required to report what he saw.

(3) Simple perceptual material combined with a simple motor task: The subject was required to trace with a stylus a simple white maze pattern on a black background.

Since the acuity in fully dark-adapted vision is low, and only large objects, simple outlines and strong brightness contrasts are visible, it was necessary to present fairly large, boldly designed black and white forms such as those described.

A further point arises in connexion with individual differences in the absolute threshold of vision during dark adaptation. It has usually been assumed that if one individual can see much better than another immediately after coming out from a brightly lighted room into the dark street, then the same individual will also see much better than the other after being in darkness for a long period of time. Now the ability to see in the dark for a short period after the eye has been bright adapted has been shown by Hecht (1921), Kohlrausch (1922), and Hecht, Haig & Chase (1937) to be a function of cone vision entirely. But the amount which the cones can dark adapt is limited, and becomes complete after 5–10 min. in darkness even when the eye has previously been completely adapted to illuminations of very high intensity (sooner, of course, after partial light adaptation). If, however, the individual is not in complete darkness, or is frequently exposed to lights of moderate intensity, he will continue to make use of cone vision over longer periods. But if he remains in darkness, with his eyes exposed to an illumination of less than about $\frac{1}{500}$ e.f.c., he will make use of rod vision, which continues to adapt over a longer period in darkness, and does not become even approximately constant in threshold value until after 30 min. in darkness, if the eye was previously completely light adapted. Thus it may be that certain individuals have relatively low absolute thresholds in cone vision, and can therefore see fairly well just after coming out of a light room, or in moderately dim light; but they may be relatively poor in their ability to see after long periods in the dark because their absolute thresholds in fully dark-adapted rod vision are relatively high. Or the opposite condition may occur. Thus it seemed to be important to measure both absolute threshold for vision and perceptual ability more or less continuously over a longish period of time, in order to determine whether such differences existed.

II. METHOD OF EXPERIMENT

Series I. In the first series of experiments continuous determinations were made of the absolute threshold for vision during dark adaptation over a period of 50–55 min. Dark adaptation was preceded by a 3 min. period of light adaptation to a 100 W. lamp seen binocularly in Maxwellian view,[1] giving a uniform brightness of 12,000 e.f.c. over a pupil area 2·2 mm. in diameter (56° of the retina). It has been shown by Craik & Vernon (1941) that the eyes were completely light adapted by this degree of brightness, and thus that all subjects began their period of dark adaptation in a similar state of adaptation whatever their exposure to light or darkness previous to the light adaptation.

The essential features of the apparatus used in the first series of experiments were the light-adaptation apparatus and the projector apparatus which projected a light patch on to a white screen; the brightness of the patch could be adjusted continuously by the subject to threshold intensity.

The light adaptation apparatus consisted of a metal box containing the 100 W. lamp at the farther end. This was observed binocularly through adjustable condenser lenses which gave the Maxwellian view. Infra-red radiation was cut out by a calorex filter. At the close of the period of light adaptation, the box was swung rapidly out of the field, and a pair of artificial pupils, 2·2 mm. in diameter, rigidly connected with the box, were swung in front of the subject's eyes. Through these he viewed the test patch on the screen. The position of the lenses, the pupils and the chin rest was correctly adjusted before light adaptation began.

The projector apparatus consisted of a 3 W. bulb in a light-tight box with condenser and objective in a focusing mount. The test patch was 5 in. in diameter, on a screen 50 in. from the subject; the visual angle was therefore 5° 43′. The maximum brightness of the test patch was 0·27 f.c.; the brightness at the eye, 0·22 e.f.c. A neutral gelatine wedge filter could be moved across the light objective by means of a rack and pinion arrangement, in order to vary the brightness of the test patch. A pencil fastened to the frame of the filter traced on a clockwork drum which made one complete revolution per hour. A continuous tracing was thus obtained of the variations in position of the filter, and hence of the brightness of the test patch, over a period of almost 1 hr. The thickness of the wedge, and hence the light intensity, varied on a logarithmic scale, over 6 log units; and a fall in the traced curve of 1 in. corresponded to a ten-fold decrease in intensity.

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[1] In the Maxwellian view the pupil and the light source are placed at conjugate foci of the lens. The eye, being near the lens, has cast upon it a much reduced image of the light source, and this acts as an artificial pupil limiting the area of the retina stimulated. A uniformly illuminated field of light is perceived.

The subject was instructed to adjust the brightness of the test patch continuously so that it always appeared to him just visible but no more. To help him in locating the test patch, which at threshold intensity was rather easy to lose, a fixation point was provided at the centre of the test patch. This consisted of a blue flash lamp bulb placed behind the screen and shining through a hole in it. Its intensity could also be adjusted by the subject so that it was slightly, but not greatly, brighter than the test patch.

Further details as to apparatus and its calibration are given elsewhere (Craik & Vernon, 1941).

The subjects of the experiment were sixteen adults, thirteen male and three female, aged 20–40. All but one had had a University education, and all were experienced in experimental work.

Series II. In Series II, the procedure during light adaptation and during the first 10 min. of dark adaptation was the same as the procedure in Series I. The diameter of the test patch was now 9½ in. (10° 45′). After 10 min. dark adaptation, the test-patch light was extinguished, and on the white screen was placed a piece of black cardboard, 10 in. square, with a white dial hand, 4½ in. long by ¼ in. wide, rotating about its centre (see Fig. 1*a*). The light was switched on again, and the subject instructed to increase its intensity until he could perceive the dial hand and report its position in terms of the hour hand of a clock. White patches on the four sides of the square enabled the subject to orientate his perception of the dial hand; but the latter was never set to coincide with any one of the patches.

Eight readings of the dial hand were made. Again after 20 and 40 min. dark adaptation further sets of eight readings were made. Between making these readings, the subject continued adjusting the test-patch brightness to the absolute threshold. Fifteen of the same subjects carried out this series of experiments, together with two additional subjects who had not had a University education nor any training in experimental work.

Series III. In this series the procedure during the light adaptation and during the first 10 min. of dark adaptation was as in Series II. The test-patch light was then switched out, and a black and white silhouette picture, 10 in, square, placed on the white screen (a specimen is shown in Fig. 1*b*). The experimenter then moved the filter back through 0·5 in. in order to increase the intensity of the test-patch light by 0·5 log units above threshold (about three-fold). The light was switched on for 2 sec. and the subject required to report what he saw. The filter was then moved back a further 0·5 in. (a total increase of ten-fold), and a further 2 sec. exposure given. The light was then turned on and the subject allowed to view the silhouette at his leisure. Finally, if he was still unable to perceive it at all correctly, he was instructed to increase the light intensity until he could do so.

A silhouette was presented to the subject every 5 min. from 10 to 45 min. after the beginning of dark adaptation, the above procedure being repeated each time. In between presentations the subject continued to adjust the test patch light to the absolute threshold. The subjects were the same as in Series II.

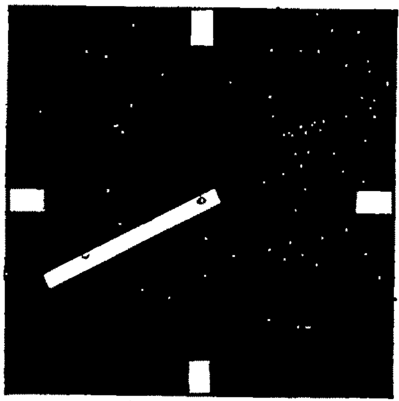

Fig. 1 *a*.

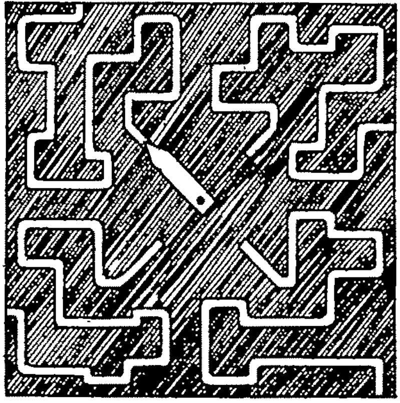

Fig. 1 *c*.

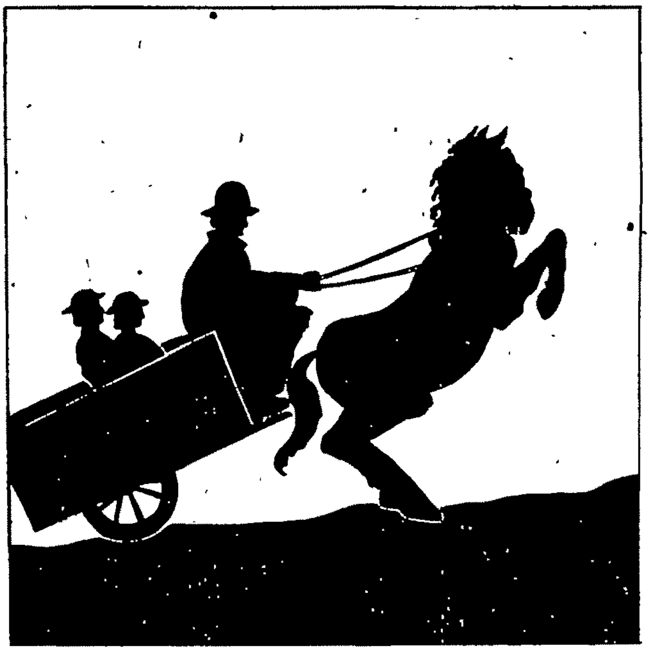

Fig. 1 *b*.

Series IV. In this series the intensity of the light during light adaptation was 8000 e.f.c. It has been shown by Craik & Vernon (1941) that the eye is so nearly completely light adapted by a light of this intensity that the form of the subsequent dark adaptation curve is almost identical with that obtained after adaptation to a light of 12,000 e.f.c. After light adaptation the subject was seated in the dark 2 ft. from a vertical blackened metal plate, 20 in. square, which he observed binocularly and without artificial pupils by the light of two flash-lamp bulbs shining through pin-hole apertures, placed behind and on either side of him. On the four quadrants of the plate were pasted four white paper maze patterns (see Fig. 1*c*); and in the centre was a white pointer which could be set to point to any one of the patterns. The intensity of illumination of these was 0·0007 f.c., giving a brightness at the eye of 0·0005 e.f.c. At first the patterns were invisible to the subject, but they appeared gradually, and after a period of 7–20 min. he could see the pointer. He was instructed that as soon as he could do so he should report its direction, and, if he did this correctly, begin to trace out the corresponding maze pattern with a metal stylus. If he allowed the stylus to slide off the pattern on to the metal plate, electric contact was made, which was recorded on an electrically operated drum. A time marker also recorded on the drum, and a signal, operated by the experimenter, at the beginning and the end of the maze tracing. The tip of the stylus was coated with radium paint, so that it was faintly but not clearly visible to the dark-adapted eye. As soon as the subject finished the first maze, the pointer was reset, and 3 min. after the commencement of the first maze he was required to begin the second. The third and fourth mazes were begun 6 and 9 min. respectively after the beginning of the first maze.

The subjects were the same as before.

III. RESULTS OF EXPERIMENTS

(1) *Series I*

The curves for the decrease with dark adaptation of the absolute threshold for vision all show the same general form which is indicated in Fig. 2 (p. 213). During the first period when the curve falls rapidly cone adaptation was taking place. After that there was a stationary stage, or even a rise of threshold, until rod adaptation began. The latter was a less rapid process, coming gradually to a standstill. The greater part of it was complete after about 30 min. in the dark, but, at least in certain individuals (e.g. Subject A, Fig. 2), still continued at a slow rate up till the end of the 55 min. period.

There were considerable individual variations in the rate of fall of the curve; in the absolute thresholds for fully dark-adapted cone and rod vision; and in the period of time until rod adaptation began to appear. The final cone and rod thresholds for the different subjects are given in Table 1, together with the time until the commencement of rod adaptation; and typical curves for four subjects are shown in Fig. 2. For clearness of presentation these have been 'smoothed' from the original tracings, but examples of parts of the latter are given in Fig. 3. Subject D in Fig. 2 had a very low cone threshold, attained very rapidly; and rod adaptation began after 5 min. Subject A had a rather high cone threshold, and rod adaptation did not begin until after 10 min. But the rod threshold fell rapidly, and reached the same level as Subject D's after 30 min., and finally a lower level than Subject D's. The cone threshold of Subject P was almost the same as that of Subject A, but he was very slow in reaching the beginning of rod adaptation, and his final rod threshold was far higher than A's. Subject O's cone threshold was much higher than that of any other subject in the series;[1] but his rod adaptation began quickly, after 6 min., and his final rod threshold was considerably lower than Subject P's. Thus Subject A showed a great capacity for rod adaptation, covering 2·6 log units (about 400-fold); while Subject P showed a very small capacity, covering only 1·0 log units, or ten-fold. Subjects D and O were intermediate in range. And Subjects D and P would tend to 'see in the dark' relatively better immediately after coming out of the light; while Subjects A and O would do relatively better after a long period of time in the dark. But Subject O would never reach the capacity of Subjects A and D to 'see in the dark'. Consequently in estimating the ability to 'see in the dark' it is necessary to take into account the exact situation in which 'seeing in the dark' is to take place.

Table 1

	Intensity in log units of		Commencement of rod adaptation in min.		Intensity in log units of		Commencement of rod adaptation in min.
Subject	Final cone threshold	Final rod threshold		Subject	Final cone threshold	Final rod threshold	
A	−2·1	−4·7	11	I	−2·4	−4·1	9
B	−2·7	−4·6	4	J	−2·4	−4·0	9
C	−2·1	−4·5	8	K	−2·2	−3·8	6
D	−2·9	−4·5	5	L	−2·0	−3·8	10
E	−2·6	−4·2	8	M	−2·2	−3·7	10
F	−2·3	−4·2	8	N	−2·0	−3·65	12
G	−2·3	−4·2	12	O	−1·5	−3·6	6·5
H	−2·3	−4·2	8	P	−2·2	−3·2	17

When the actual tracings of the threshold readings are studied, a further characteristic difference between subjects appears. The subject was instructed at the beginning of the experiment that the brightness of the test patch might seem to fluctuate, so that the test patch might

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[1] Actually Subject O's cone threshold was rather variable; but Subject L gave an example of a consistently high cone threshold (see Fig. 4).

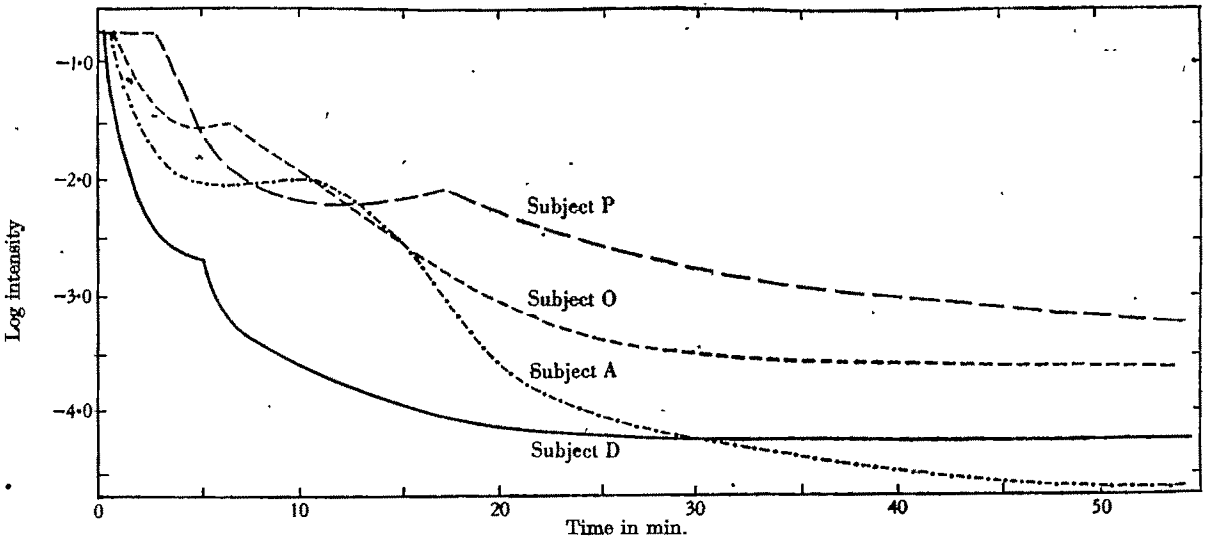

Fig. 2. Individual dark adaptation curves.

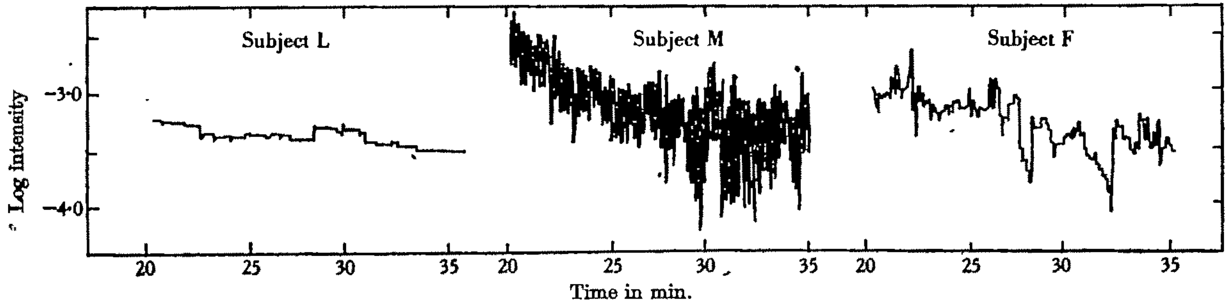

Fig. 3. Individual variations in dark adaptation (unsmoothed) curves.

perhaps disappear altogether, and in consequence if he wished he might bring it back into view by increasing the illumination. Some subjects made little use of this permission, others a great deal. Thus some curves were relatively smooth, while others showed constant fluctuation. Moreover, the fluctuations were greater in extent in the latter than in the former. Portions of three such curves given by Subjects L, M and F are shown in Fig. 3. Now it would appear that in perception Subject L might do much better or much worse than Subjects M and F according as a momentary glance or a steadily maintained scrutiny was required. We shall show below that something of the kind did appear in the perceptual experiments; and that an underlying temperamental difference seemed to account for these differences in fluctuation, and for the corresponding differences in perceptual ability.

(2) *Series II and III*

From the results of Series II two curves were obtained for each subject, one for the absolute threshold of vision and one from the points at which the dial hand was perceived. The curve for the absolute threshold was also obtained in Series III, but not for the points at which the silhouettes were perceived, since these were presented at intensities bearing a fixed ratio (3 and 10 times) to that of the absolute threshold. Another method of scoring perceptual ability was therefore adopted, which will be described below.

It is interesting in the first place to compare the final absolute threshold for rod and cone vision in Series I, II and III. The use of the larger test patch in Series II and III resulted in a lower final rod threshold; this effect was produced presumably by summation in rod vision. But the thresholds for the three series showed a fairly close parallelism.[1] The final cone and rod thresholds for Series II and III are given in Table 2, and in Fig. 4 they are plotted against the corresponding values for Series I. The chief discrepancies are shown by Subjects F and G, and to a less degree by B and Q. It was quite clear from their behaviour that F and G were erratic subjects, who easily became bored and inattentive under the rather monotonous conditions of the threshold experiments. Series II and III provided more variety than did Series I, and stimulated them to work more accurately. The type of curve reproduced for Subject F in Fig. 3 seems to be characteristic of such an observer; it shows a constant unsteady fluctuation, though without the almost obsessive to-and-fro movement of Subject M's. It seemed probable that B also was rather inattentive, though not restless and bored. Q was an inexperienced and untrained subject, and it is probable that in Series II, the first he performed, he kept the test patch well above the threshold intensity, but reduced it somewhat in Series III. Confirmatory evidence of this point will be given below. It may be concluded, however, that the majority of individuals will give fairly consistent and reliable results in an absolute threshold experiment. But there will also be certain individuals who are not reliable; and lack of training and tendency to feel boredom may decrease reliability.

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[1] The degree of correspondence between these and subsequent sets of values for the different subjects might be measured by correlating them. But such a measure would indicate little. The number of subjects is too small to deduce any general trends from their results; and individual differences would be obscured, whereas they are revealed by comparison of the curves.

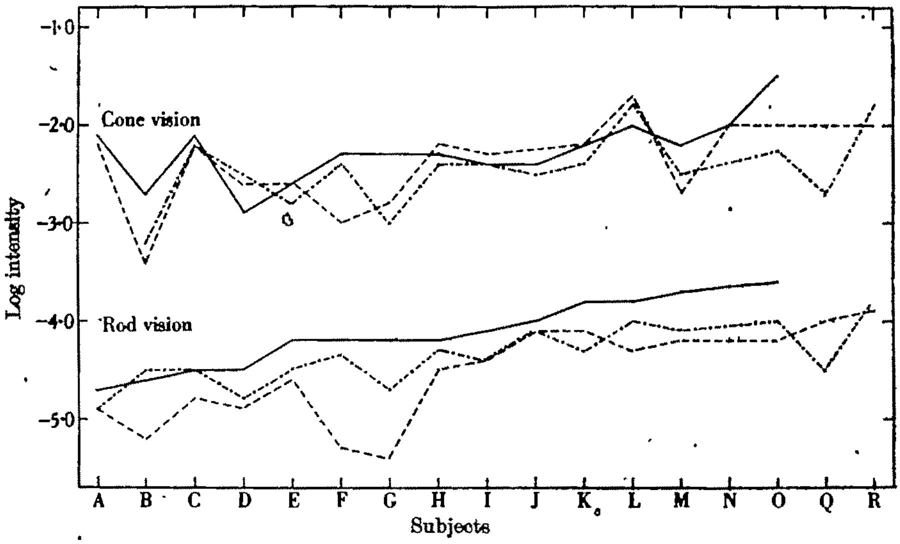

Fig. 4. Final absolute thresholds in cone and rod vision.
——— Series I. – – – Series II. –·–·– Series III.

More important, of course, is the relationship between the perception of form and the absolute threshold of vision. In Table 3 are given the average intensities at which the dial hand was perceived during the periods beginning 10 and 40 min. after the commencement of dark adaptation; and the corresponding average absolute thresholds during those periods. The same results are plotted in Fig. 5. Here the correspondence is extremely close during the first period in which mainly cone vision was employed—for perception of the dial hand almost entirely. This is rather to be expected, since during this period acuity was fairly high, and so there was little necessity for using the interpretation from faint visual cues which we shall find to be so important in certain types of perception. The correspondence is rather less striking during the

Table 2

	Series II		Series III	
Subject*	Final cone threshold	Final rod threshold	Final cone threshold	Final rod threshold
A	−2·2	−4·9	—	−4·9
B	−3·4	−5·2	−3·2	−4·5
C	−2·2	−4·8	−2·2	−4·5
D	−2·6	−4·9	−2·5	−4·8
E	−2·6	−4·6	−2·7	−4·5
F	−3·0	−5·3	−2·4	−4·35
G	−2·8	−5·4	−3·0	−4·7
H	−2·2	−4·5	−2·4	−4·3
I	−2·3	−4·4	−2·4	−4·4
J	—	−4·1	−2·5	−4·1
K	−2·2	−4·1	−2·4	−4·4
L	−1·7	−4·3	−1·8	−4·0
M	−2·7	−4·2	−2·5	−4·1
N	−2·0	−4·2	—	—
O	−2·0	−4·2	−2·25	−4·0
Q	−2·0	−4·0	−2·7	−4·5
R	−2·0	−3·9	−1·8	−3·8

* Certain subjects, or certain of their results, are omitted from this or subsequent tables, because they were unable to perform the series, or because the record of their performance was imperfect.

Table 3

	After 10 min. dark adaptation			After 40 min. dark adaptation		
Subject	Av. intensity for perceiving dial hand	Av. absolute threshold of vision	Difference	Av. intensity for perceiving dial hand	Av. absolute threshold of vision	Difference
A	−1·9	−2·7	0·8	−3·7	−4·7	1·0
B	−3·9	−4·45	0·55	−4·4	−5·2	0·8
C	−2·7	−3·2	0·5	−4·0	−4·85	0·85
D	−1·8	−2·9	1·1	−3·4	−4·85	1·45
E	−2·5	−3·05	0·55	−3·7	−4·5	0·8
F	−3·2	−3·8	0·6	−4·0	−5·25	1·25
G	−3·05	−3·55	0·5	−3·9	−5·1	1·2
H	−2·4	−3·15	0·75	−3·7	−4·6	0·9
I	−2·4	−3·05	0·65	−3·7	−4·35	0·65
K	−1·9	−2·75	0·85	−3·1	−4·1	1·0
L	−1·8	−2·4	0·6	−3·5	−4·3	0·8
M	−2·2	−2·8	0·6	−3·3	−4·2	0·9
N	−1·8	−2·3	0·5	−3·1	−4·05	0·95
O	−2·6	−3·35	0·75	−3·1	−4·2	1·1
Q	−2·1	−2·4	0·3	−3·7	−4·0	0·3
R	−2·0	−2·45	0·45	−3·7	−3·8	0·1

second period, though it is still fairly close. Subjects D, Q and R are somewhat anomalous. In the case of Subject D, this discrepancy was due to his very considerable refractive errors (his vision was −2·75D. for the right eye, −11·5D. for the left), which greatly reduced his ability to perceive forms under the conditions of these experiments. As mentioned above, Subjects Q and R were inexperienced, and for this reason kept the test patch at an illumination well above threshold during absolute threshold measurements; hence the intensity of the latter was very little lower than that of the dial hand readings. But we may conclude that in general the ability to perceive in dim illumination such a simple type of object as a white dial hand on a black background is mainly a function of the absolute threshold for vision under corresponding conditions.

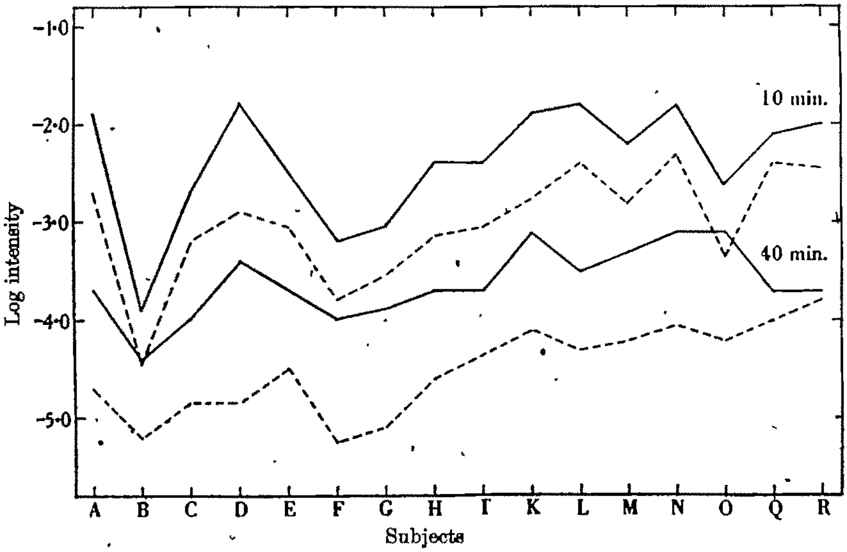

Fig. 5. Average intensity for perception of dial hand and average absolute threshold 10 and 40 min. after beginning of dark adaptation. ——— Perception of dial hand. – – – – Absolute threshold.

But in perceiving such an object, the 'degrees of freedom' possessed by the observer are few. He knows exactly what type of form he will be required to perceive, and that its only variation will be its rotation round a circle. But in the perception of other forms the possible variations are much more numerous, since their size and shape may be quite unknown beforehand to the subject. If a collection of real objects is presented, he must not only perceive their shape—he must in addition recognize what they are. Now in very dim light, the intensity of which is below the threshold for cone vision (about $\frac{1}{500}$ e.f.c.), it is impossible to make out any detail of the form of an object, since the acuity of rod vision is too low (Marshall, 1939). Thus the observer will get no more than a vague impression of the shapes of objects. In the same way, if he is shown meaningful pictures, he will see only general contours and strong brightness contrasts. From these he must not only gain some general idea of the shapes depicted, but must also give these a meaningful interpretation, and must recognize what the pictures are intended to represent. It is possible that the perception of objects, and the abilities required for it, may differ in nature from the perception of pictures. But the sensory data involved in the two types of perception are much more similar in dim than in bright light, since only general impressions, without any detail (or, probably, any impression of depth and solidity, in the case of solid objects), can be gained in either case. Consequently in both cases the observer is obliged to fill out the vague sensory cues and interpret them in accordance with any previous knowledge he possesses, any anticipation of what is likely to be seen, and any internal consistency in the picture itself.

Thus it is natural to expect that the ability to perceive objects or meaningful pictures will not be determined wholly, or even mainly, by the absolute threshold of vision. And therefore Series III, consisting of meaningful silhouette pictures, was presented in the hope that it might throw some light on the other important factors in the process. Now it was not possible to proceed as in Series II and allow the subject to increase the illumination until he could say what the pictures were. The point at issue was to determine the lowest intensity at which it was possible to make a reasonably accurate guess at the meaning of the picture. Left to himself to increase the intensity, the subject would usually increase it until details of the pictures were fully visible, before making his report. Consequently the subject was first of all required to say what he thought the picture represented when the latter was exposed at an intensity of three, and then ten, times the visual threshold; first for a short, 2 sec. exposure, and after for a long exposure. Finally, if he still had no idea what the picture was, he was allowed to increase the intensity until he could make it out.

It appeared immediately that the subject was frequently able to get a very good idea of what the pictures represented even when the illumination was very dim, i.e. below the cone threshold. If, however, he was for any reason unable to make this sort of guess, but required to see further detail in order to make out the picture, he was obliged to increase the intensity above the cone vision threshold. That is to say, although rod vision possessed an acuity too low to allow any accurate perception of detail, it did enable the subject to make out the general nature of the picture, provided he could fill out the vague sensory cues from some sort of idea as to what they indicated. It was most striking to note the accuracy of these interpretations in view of the vagueness and uncertainty of the actual sensory impressions which the subjects reported. Some idea of the way in which decisions were taken about vague perceptual impressions is given by the following reports:

> "A man walking up hill and an animal in front of him—awkward and strained position—so peculiar he mightn't have been a man."
>
> (First report): "A series of what looked like pillars." (Second report): "Four objects; the highest has a sharp peak to it, might be a spire; the other ones square, might be houses."
>
> "A man with two either dogs or sheep. Looked as if he might be holding on to one, therefore presumably it was a dog."

It was also possible to measure the least intensity at which these pictures could be perceived from the lowest intensity at which any one of the silhouettes was accurately perceived. Another measure was given by the lowest intensity at which any one of the silhouettes was partially perceived, i.e. the subject had a general idea as to the type of object or

Table 4

Subject	Final rod threshold	Lowest intensity at which silhouettes were perceived		Av. intensity for perceiving silhouettes in cone vision
		Partially*	Wholly*	
A	−4·9	−3·9	−3·9	—
B	−4·5	−3·25	−2·95	−2·3
C	−4·5	−3·5	−3·15	−2·1
D	−4·8	−2·35	−2·05	−1·5
E	−4·5	−3·4	−3·4	−2·3
F	−4·35	−3·8	−3·05	−1·8
G	−4·7	−3·6	−3·0	−2·6
H	−4·3	−3·2	−3·0	−1·7
I	−4·4	−3·85	−3·3	−1·9
J	−4·1	−3·35	−3·35	−2·1
K	−4·4	−3·8	−3·25	−1·9
L	−4·0	−3·35	−2 95	−1·7
M	−4·1	−3·05	−3·05	−2·4
O	−4·0	−2·95	−2·3	−1·5
Q	−4·5	−3·3	−3·3	−2·2
R	−3·8	−2·75	−2·1	−1·8

* When the figures under 'partially' and 'wholly' are identical, this indicates that the subject was able to perceive the silhouette 'partially' during the 2 sec. exposure at this intensity, and 'wholly' during the long exposure.

scene depicted, but could not report it accurately. These figures are given in Table 4, together with figures for the final rod thresholds in this Series; and are also plotted in Fig. 6, in descending order of absolute threshold. It is immediately clear that there are considerable discrepancies between the ability to perceive the silhouettes and the final rod threshold. It is true that on the whole those with lower rod thresholds are superior in perceptual ability to those with higher rod thresholds. But there is a smaller range of perceptual ability than of absolute threshold; or, to put it another way, those with low rod thresholds are less superior in perceptual ability than they are in visual ability to those with high rod thresholds. This is probably due to the low visual acuity of dark adapted rod vision; thus in perception, which demands a certain degree of visual acuity, those with low rod thresholds do not obtain the full advantage of this quality.

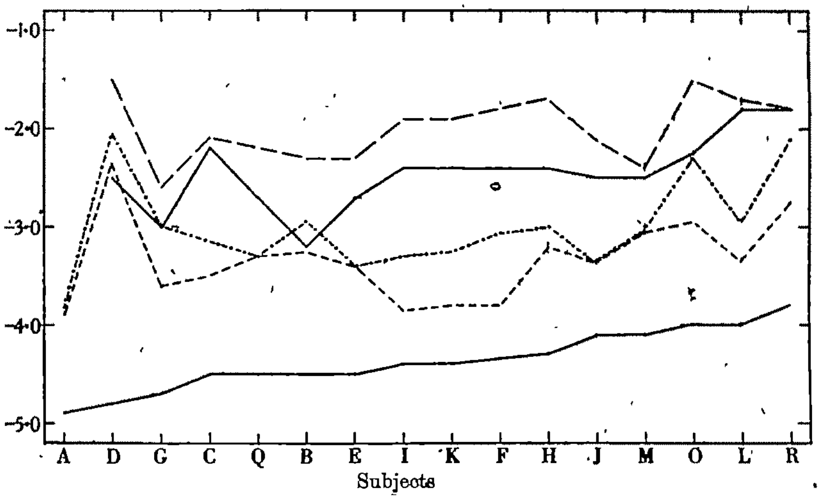

Fig. 6. Intensity for perception of silhouettes and absolute threshold. ——— Final cone and rod thresholds. — — — Average intensity at which silhouettes were accurately perceived in cone vision. ------ Lowest intensity at which silhouettes were accurately perceived in rod vision. —·—·— Lowest intensity at which silhouettes were partially perceived in rod vision.

But there are also individual variations. Subject D was conspicuously poor at perception; and this we have already mentioned as being due to his refractive errors. Minor refractive errors may have accounted in part for Subject O's poor perceptual ability; but we shall refer to him again below. Other subjects who were relatively poor at perception were B, G and R. Subjects B and R were quite definitely inferior both in intelligence and in general education to the other subjects of the experiment; and they made relatively little attempt at an intelligent interpretation of the 'partial cues' afforded by the vague visual impressions. Thus for instance Subject R reported seeing 'some sort of tall structure like a spire', but made no further attempt at inferring that the picture might contain a church.

Subject G was conspicuously poor throughout Series III, although his score pulled up a little by an accurate perception of the last silhouette. He was one of those cautious and resistant individuals who hate to give themselves away or be caught napping and he refused to 'guess at' any of the possible interpretations. Subject H was also relatively bad at perception, though less so than those just mentioned. A biologist by profession, he adopted throughout a cautious scientific attitude towards interpretation of partial cues. His responses were brief and his guesses few, and he obviously did not wish to report anything for which he had no accurate evidence.

Similar attitudes were apparent among some of the subjects who could not partially perceive the pictures at an intensity any less than that at which they could perceive them accurately. These were Q, E, J and M. E's attitude was similar to that of G, though less extreme. J and M, on the other hand, were over-anxious, afraid lest they might be mistaken, and therefore careful only to report what they could be certain of. This anxiety and uncertainty were also demonstrated in their absolute threshold curves, which were of the constantly fluctuating kind reproduced in Fig. 3. Q's deficiency was due partly to anxiety, but also to lack of training in this type of experiment.

Quite opposite were the attitudes of Subjects F, K, L and O. They were willing to 'take a chance on it' and make guesses in the hope that these were correct. In this L was conspicuously successful. He was unusually 'quick on the uptake' and adept at making the most of his rather poor visual equipment. But I and K were less successful. That is to say, their ability to perceive accurately was about what would be expected from their visual equipment; and their readiness to guess gave them only vague and not accurate ideas of what was presented.

With Subjects F and O this guessing was probably deleterious. F has already been referred to as the subject giving the fluctuating curve in Fig. 3, whose absolute threshold varied a good deal from day to day, becoming lower in the more interesting and higher in the more boring experiments. At the same time he exhibited a rather wild and reckless attitude to the silhouette perception, and was ready to advance guesses at interpretation on quite inadequate grounds. Moreover these responses were over-determined by the content of the previous presentations, a characteristic also appearing in Subject O's responses, though less conspicuously. It is shown clearly in the tabulation of Subject F's responses in Table 5. He was a highly suggestible individual, and there is little doubt that he was temperamentally a poor observer in this type of experiment.

Table 5

Silhouette picture presented	First interpretation*
Man, horse and cart; horse has a flowing tail	North of Scotland
Ship at sea	A dog with a *long tail*
Two men and a dog	*Two sailing ships at sea*
Church with tower, and trees	Factory chimneys; or Stonehenge
Man, a sheep and a lamb	Animal, *wheeled thing*, man
Man and a fox	Man on *horseback*; on a cliff by the *sea*
Church with spire, houses and a tree	Buildings
Yachts at sea	Islands with *trees*

* Interpretations derived from previous presentations are in italics.

It is also of some interest to study the relative perceptual ability of the subjects in cone vision, when they were able to see the details of the pictures with fair accuracy. A measure of the ability was given by averaging the intensities at which the subject set the illumination when he was required to increase it until he could see what the picture was (only those values have been averaged which were above the final cone threshold and which corresponded to an accurate perception of the picture). These figures are given in Table 4, and are plotted in Fig. 6 together with the figures for the final cone thresholds in this series. Two things are clear. There is a much closer correspondence between final cone threshold and perceptual ability in cone vision than there is between final rod threshold and perceptual ability in rod vision. There is a slighter correspondence between perceptual ability in cone and in rod vision; but this is greater than the correspondence between final rod threshold and perceptual ability in rod vision. In other words, there is a tendency for perceptual ability to remain relatively constant throughout the cone and rod periods of dark adaptation, and independent of the absolute threshold; but it is affected to a greater extent during cone than during rod vision by the absolute threshold. Now since perception during cone vision is in part dependent on perception of the details of the pictures, it requires the relatively high acuity of cone vision, and thus perceptual ability will be in part a function of the cone threshold. But certain subjects showed a considerable discrepancy between this ability and the final cone threshold—in particular Subjects B, D, H, and O were relatively poor in perception, Subjects M and R relatively good. Now Subject B had a very unusually low cone threshold; but his perceptual ability has already been mentioned as rather inferior. But it seems that perceptual ability in cone vision was less dependent on intelligence than was perceptual ability in rod vision, since inference was much less in use. This is confirmed by Subject R's much better performance in cone vision. The fact that the average intensity for cone perception of this subject coincided with his absolute threshold was due to his tendency, already described, to set the absolute threshold too high. Subject Q's threshold readings were also too high, for the same reason.

The relatively poor perceptual ability of D, H, and O has already been commented on. It is interesting to note the improvement of Subject M. It appears that with the relatively well-defined sensory impressions of cone vision the anxiety caused by guessing was no longer operative; and a natural quickness in noticing and making use of detail in the pictures enabled him to perceive them strikingly well—better than any other subject except G.

It is clear then that the individual with a relatively low cone threshold will have a considerable advantage in perceiving any object sufficiently illuminated to exceed this threshold. On the other hand, the individual with the relatively low rod threshold will not have the same advantage in perceiving objects at lower intensities. But psychological abilities may qualify a low rod threshold and to some extent compensate for a high one. They will have less effect on this kind of cone vision.

(3) *Series IV*

In Series IV the perceptual task was again relatively simple, approaching that of Series II; but it was complicated by the accompanying motor task of tracing the maze pattern. It is true that this task involved very little manual dexterity, and in a good light the subjects could have performed it without making any errors. But the perceptual task was nevertheless made harder by the fact that it was to some extent doubled; that is to say, the subject had not only to observe the direction of the maze pattern (which changed frequently and was not uni-directional as was the dial hand); he also had to perceive, even if only vaguely, the position of the stylus, in order that the latter could be made to coincide with the former.[1]

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[1] Unfortunately, the ability to locate the position of the end of the stylus kinaesthetically from the position of the hand varied considerably in different subjects; thus some made much more use of kinaesthetic cues; others of purely visual ones. Obviously the former were at an advantage.

But in one respect this test gave a measure of perceptual ability which was very similar to that obtained in Series II—namely, the time until the subject could perceive the orientation of the white pointer which was set to point to the beginning of the first maze pattern. This time varied from 7½ to 19 min. for the various subjects. Thus the relative ability of the subjects in this test might be expected to correspond to their relative ability in accuracy of perception of the dial hand at the periods either 10 or 20 min. after the beginning of dark adaptation. In Table 6 the three sets of figures are given; and they are plotted in Fig. 7 in ascending order of time interval for Series IV. We see that there is a correspondence between the sets of values, though it is not as close as that between the average intensity for the dial hand readings and the absolute thresholds in Series II. But an exact correspondence would not be expected, since Series II was performed with artificial pupils, Series IV with natural pupils. We cannot therefore draw any conclusions from the discrepancies between the two curves; but we are probably justified in assuming that the times taken to see the pointer in Series IV give an adequate measure of the same type of simple perceptual ability as was given by the average intensity for the dial hand readings in Series II.

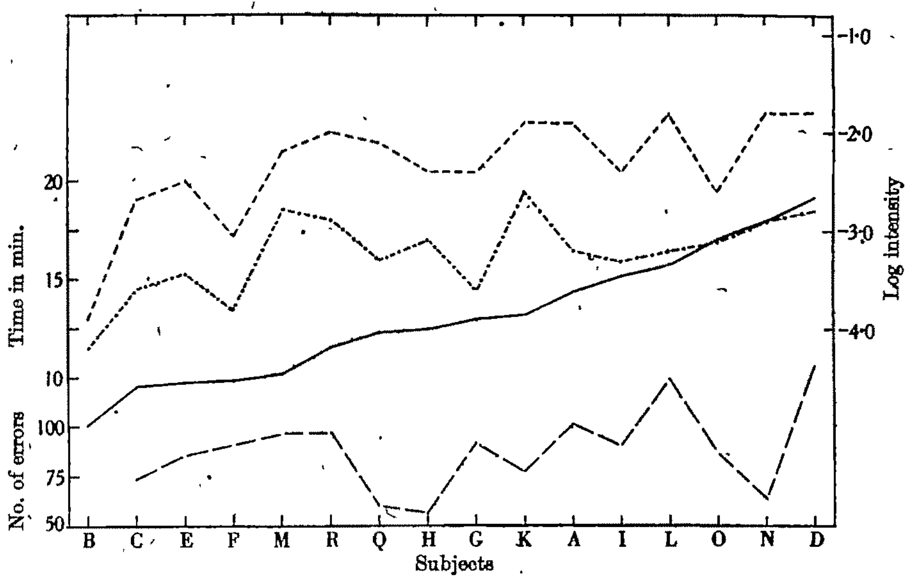

Fig. 7. Perception of pointer and number of errors in maze tracing. ——— Time until pointer was perceived in Series IV. — — Number of errors in maze tracing in Series IV. ------ Average intensity for perception of dial hand after 10 min. in Series II. —·—·— Average intensity for perception of dial hand after 20 min. in Series II.

Table 6

	Series IV		Series II Av. intensity for perceiving dial hand	
Subject	Time till pointer was perceived	No. of errors	After 10 min.	After 20 min.
A	14·7	101	−1·9	−3·2
B	7·6	—	−3·9	−4·2
C	9·5	73	−2·7	−3·6
D	19·1	133	−1·8	−2·8
E	9·8	86	−2·5	−3·45
F	9·9	91	−3·05	−3·8
G	13·5	92	−2·4	−3·6
H	12·5	56	−2·4	−3·1
I	15·3	91	−2·4	−3·3
K	13·7	78	−1·9	−2·6
L	15·8	125	−1·8	−3·2
M	10·2	97	−2·2	−2·8
N	18·0	64	−1·8	−2·9
O	17·1	88	−2·6	−3·1
Q	12·3	60	−2·1	−3·3
R	11·6	98	−2·0	−2·9

The accuracy of the maze tracing in Series IV was measured by the number of errors—times that the pointer slipped off the maze on to the metal plate. These are given also in Table 6, and plotted in Fig. 7. Again there are discrepancies between the times until the pointer was perceived, and the number of errors in tracing. Subjects H, N, O and Q showed conspicuously few errors; Subjects L and D, possibly also M and R, an unduly large number. The subjects could of course regulate their own speed of tracing, but in no case shown here was frequency of errors due to high speed of tracing; those who traced slowly made more errors than those who traced more rapidly.[1] It seems that Subjects H and Q were as careful in their performance of this Series as they were in Series III; and Subjects N and O here showed a similar type of caution which did not appear in Series III. The frequency of errors shown by Subjects L and D was probably due to their poor visual acuity. Subject M's over-conscientiousness here had an effect opposite to that which occurred in Series III; he was so anxious not to be late in reporting the position of the pointer that he went to the other extreme and did so while it was still very faint and indefinite. Thus when he began to trace the first maze he could see it only very faintly, and was thus slow and inaccurate.

[1] Subjects J and P were possible exceptions to this generalization; they are not included here because they did not do Series II, which was carried out later than Series IV. Subject P's very large number of errors may have been partly, but not entirely, due to his fairly rapid speed; and Subject J was slow and accurate. But these two cases seemed exceptional.

On the whole the results of this series are rather disappointing, probably because of the impossibility of controlling the experimental conditions rigidly. There is some indication, however, that caution and anxiety, as well as the absolute visual threshold, may determine the accuracy of the performance. But they may affect it in a different manner from that in which they affected performance in Series III, which is explicable in view of the different psychological nature of the abilities involved.

IV. Discussion

It is fairly clear, then, that the psychological basis of the perceptual performance in dim light, and hence the qualities required for it, may differ considerably in different perceptual tasks, and, indeed, may be almost specific to a particular task. There is no doubt, also, that familiarity with the situation and task is an important factor in assisting perception; and such familiarity again may be specific for the material, the situation and the particular observer. For instance, we have noted that the inexperience of Subjects Q and R affected their performance both in Series II and III. Again, in Series III, it was observed that pictures of men, objects which are familiar to most people in silhouette pictures, were always more readily perceived than the less familiar pictures of scenery. In particular, a picture of an Italian church, relatively strange to most observers, was generally found the hardest to perceive.

It follows, then, that it is exceedingly difficult to lay down any broad general conclusions as to the qualities and abilities which make for 'being able to see in the dark'. Of course it is undeniable that low absolute thresholds, both in cone and in rod vision, are of importance. Subject P, for instance, had a very high rod threshold. Unfortunately he was unable to carry out Series II and III, so that the effect of this on the perception of silhouettes could not be measured. But in Series IV his time to see the pointer, 18·2 min., was longer than anyone's except Subject D's, and his number of errors was the largest of all, 169. And there is little doubt that, however favourable his psychological qualities, he would have been greatly handicapped even in Series III. Other individuals, such as Subjects L and O, were relatively poor in seeing in moderately dim light, or just after coming out of a light room, because their cone vision thresholds were relatively high; but they were not so poor in seeing after a long period in the dark. Again, a high degree of refractive error may co-exist with a low absolute threshold, as in the case of Subject D, and may be highly deleterious to perception in the dark.

But on the whole the more complicated becomes the perceptual material and situation, and the vaguer the sensory data, the greater the extent to which perceptual ability is determined by psychological as well as by physiological factors. These psychological factors are both cognitive and affective, innate and acquired. The following are those discovered in the course of these experiments; but there may well be others which would appear in a more exhaustive enquiry with more subjects:

Cognitive factors. There is no doubt that *intelligence* plays an important part in helping the subject to 'guess at' the possible interpretations to be placed upon the incomplete sensory data, and to select those probable and relevant to his knowledge of the general situation. Subjects B and R showed quite clearly their inability to 'think of' possible meanings for the silhouettes. On the other hand in cone vision, where guessing played less part, intelligence was less important.

Education and general *experience* were of importance in directing the subject towards the best use of his natural abilities. They increased the ability to select the best interpretations. Examples of this type of selection were given on p. 219. But education and experience of a particular kind, namely the highly exact and scientific, seemed in tasks such as that of Series III to prevent the subject from exploring the full possibilities of the situation, because he could not obtain the accurate check on the sensory data on which he was accustomed to rely. This effect occurred also to some extent in cone vision, where it might be less expected. The responses of these individuals were either brief and business-like, omitting all but essentials; or gave rather long descriptions of the phenomenal appearance of the presentation, without much interpretation, thus: (First report): "Almost nothing except a few lumps in the middle; line dividing the picture above and below is slightly curved and rough." (Second report): "Completely different. One low rectangle at the left, then another, then a tall object. Something on the left. Can't identify them."

More specific experience and *familiarity* with the particular material and situation encountered were definitely helpful. Familiarity with material we have already discussed. For carrying out a task such as the continuous adjustment of a very dim light to threshold intensity, some experience in performing laboratory experiments was important. We formed the opinion that such experiments performed by untrained subjects might give quite misleading results. Either they would be variable in their readings, or they would consistently keep the intensity well above threshold so that the test patch was easily visible throughout. Such conditions would not of course obtain in ordinary life. But it appears likely that the ability to perceive certain objects in very dim light might be considerably increased by training the observer in what to look for, and in giving him practice in picking it out, in specific situations such as he might have to encounter in ordinary life.

Affective factors. These were mainly innate, though there is no doubt that any acquired condition of emotional maladjustment might enhance the effect of some of them.

Considerable effects were produced by recklessness, caution, over-caution, anxiety and over-conscientiousness. The most favourable attitude was that produced by a moderate degree of *adventurousness*, a willingness to 'take a chance' that one's guesses might be correct. But a moderate degree of caution and self-criticism was also useful because it caused the subject to examine his guesses carefully before deciding on his interpretation. Too much *recklessness*, however, made him rush to conclusions and suggest something quite improbable, or give widely contradictory reports on succeeding exposures. Thus Subject F gave the following reports on a picture of a man, a woman and a dog walking uphill:

(First exposure): "Two sailing ships, yachts, and a fairly calm sea."
(Second exposure): "A bridge or something sticking out on the left."
(Third exposure): "A man walking with a wheelbarrow and a figure behind him."
(Fourth report): "Two hikers going uphill with a dog in front."

Closely allied to this was the tendency to *over-suggestibility*, shown in a readiness to produce interpretations based in whole or in part on the contents of previously presented material (see Table 5). It is probable that an individual with such a tendency would be very easily affected by any other factors in the situation which encouraged a particular type of response, i.e. 'suggested' what that response should be.

On the other hand, too much *caution* made the subject unwilling to guess at all, in case he gave himself away. This was particularly noticeable in the rather resistant type of individual who hates to be 'caught out', and who gave brief and banal responses. A rather similar effect appeared in the *anxious* and *over-conscientious* individual who was afraid to make mistakes and thus dared only to hazard an interpretation of which he was fairly certain, and gave responses such as: "A small figure, a tall figure, a small figure—might be anything." But it was interesting to note that the deleterious effect of this anxiety and hesitancy was much less in cone vision, presumably because the subject felt a greater certainty as to the detailed shape of the picture presented. However, he showed its effect in his absolute threshold readings by the extreme fluctuation of the curve (see Fig. 3, Subject M).

Another characteristic which appeared less in the silhouette experiments with their variety and short exposures than in the longer and more monotonous experiments was liability to *boredom.* It was shown in the 'restless' type of fluctuation made by Subject F in Fig. 3, and by the increase of the absolute threshold in the duller experiments. This liability to boredom appeared in subjects both of the 'reckless' and of the 'over-cautious and resistant' type.

V. SUMMARY

Curves for the absolute threshold of vision of eighteen subjects were obtained continuously over a period of 55 min. dark adaptation, after previous complete light adaptation. Certain characteristic differences were found between the curves of the different subjects. Of these the most important were the relatively high threshold of certain individuals after cone adaptation together with a relatively low threshold after rod adaptation; while other individuals had a relatively low threshold after cone adaptation, and a relatively high one after rod adaptation. These characteristics were consistent for most subjects on repetition of the experiment. But a few subjects, particularly those liable to feel boredom, and those who had had little experience of experimental work, showed rather variable thresholds.

The ability of the above subjects to perform simple perceptual tasks during dark adaptation, such as reading the position of a dial hand, was very closely related to their absolute thresholds of vision. But the perception of more complicated material, such as silhouette pictures, was determined only partially by the absolute threshold in cone vision; and still less by the absolute threshold in rod vision. The ability of the subject to interpret the vague sensory data obtained in rod vision, and hence to make out the meaning of the picture, was affected by a variety of purely psychological qualities. Intelligence, education, and familiarity with and experience of the particular type of material and situation, were of great assistance. Affectively based attitudes were also important. Too much caution or timidity prevented the necessary amount of guessing at the meaning of the pictures; too much recklessness, and suggestibility to irrelevant influences, produced inaccurate guessing.

Attitudes of this kind also affected the performance during dark adaptation of a simple perceptual and motor task, the tracing out of a

dimly illuminated maze pattern; but the actual effects often differed from those occurring in the perception of silhouettes.

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THE DIFFERENTIAL THRESHOLD FOR THE SUBJECTIVE JUDGEMENT OF THE ELASTIC AND PLASTIC PROPERTIES OF SOFT BODIES

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I. Introduction

In an earlier paper (1) experiments were described in which the thresholds for firmness were measured for two different types of soft body, namely a truly fluid bitumen and an almost truly elastic solid (rubber). In both these materials firmness depends upon single simple physical concepts, although these have different dimensions, namely those of viscosity in the one case and of elastic modulus in the other. Such materials are unfortunately rare. The kind of soft body generally met with in everyday life shows both plastic and elastic properties and it is difficult to say what firmness really means for these materials. It must depend upon some function of both plastic and elastic properties but the exact function is as yet unknown. Classical physics deals with this problem by treating the recoverable and non-recoverable deformations independently. From the psychological point of view this is clearly unsatisfactory, for some judgement for firmness can be and is made even before the pressure has been released, that is before it can possibly be known how much of the deformation is going to be recovered.

A treatment was suggested in the paper already quoted which would avoid this difficulty but which gives a 'firmness' having variable dimensions depending on other properties of the material. Experiments on the direct comparison of materials in which firmness has different dimensions (2) show, however, that this is far from being an insuperable difficulty for the average subject. A far wider experiment, in which subjects were asked to rank eight synthetic materials with mixed elastic and plastic properties, indicated that comparisons for firmness in such materials are made on the basis of total deformation, in that the rank order obtained from the pooled results corresponded more nearly with the order for total deformation after 2 sec. than with the order for any other physical measurement made. The relationship between these two interpretations of 'firmness' will be discussed later.

Unfortunately the total deformation will depend upon the pressure applied as well as upon the time of its application. Moreover, the relationship between deformation, pressure and time will not be simple as is the case with true fluids and true solids, since such complex materials almost invariably get progressively softer the harder they are pressed (structural viscosity) and progressively harder the further they are pressed (work hardening). Thus the actual sensory data received by any subject will depend upon the way in which he handles the materials, and the 'correct' order of firmness for any given set of samples will almost certainly vary for different individuals and even for the same individual at different times.

The difficulties opposing any sort of measurement of threshold under such conditions appear at first sight almost insuperable. There are, however, some four methods open for the investigation of the subjective judgement of such 'mixed' materials.

II. INVESTIGATION BY THE METHOD OF SELF-CONSISTENCY

By far the best method for comparing the efficiency of individuals seems to be to measure the self-consistency of their judgements by getting them to rank the same set of samples a number of times. Their ability to reproduce the same rank order will depend upon at least four factors: namely, (1) sensory acuity; (2) a stable mental criterion of firmness, to overcome the dimensional difficulties when the samples differ in 'spring'; (3) ability to select suitable test conditions or methods of handling, i.e. those giving maximum differences; and (4) ability to reproduce these test conditions.

This method was used by Scott Blair & Coppen (2) to compare the abilities of people having different types of training and was found quite satisfactory for this purpose, but it did not yield data directly relating subjective to objective differences nor any figures which could be compared with the threshold figures for other materials or other senses.

III. INVESTIGATION BY THE USE OF ARTIFICIAL MATERIALS

Perhaps the most obvious and ultimately the soundest solution of these difficulties is to use some kind of self-recording material which would record the pressures, deformations and times used on each sample by each subject, from which the actual properties of the material under these conditions could be calculated. One could, for example, have recording instruments embedded in the samples, but this would mean that the sample could not be homogeneous and it is not possible at present to say what effect this heterogeneity would have. The more obvious method of pressing the sample while it rests on a balance has the disadvantage that this is an unnatural method of handling for most subjects and also the movement due to the balance is added to the movement due to the change of height of the sample.

An alternative method would be to construct an artificial material. For example, small bulbs of some flexible but non-elastic material could be filled with mercury and connected to a manometer and to a capillary leak. The height of the column in the manometer would register the pressure and the additional volume of mercury would give a measure of the recoverable deformation, the volume expelled from the capillary leak giving a measure of the non-recoverable deformation. This method would eliminate the small differences in surface properties (e.g. stickiness) frequently found to accompany small differences in firmness, and might also affect some of the skin sensations in an artificial way, but these drawbacks are probably unimportant compared with the advantages. But the war obviates, for the present, the undertaking of such a task.

IV. INVESTIGATION BY THE USE OF KNOWN PRESSURES

A third method of attack would be to train subjects to use a known pressure, the time being either previously fixed or else measured during the test. The firmness could then be measured under the same conditions. Now the capacity to reproduce the test conditions is certainly a part of the ability to judge small differences in the firmness of those materials for which this property varies with the test conditions: The capacity to reproduce a set of conditions arbitrarily fixed by the experimenter, however, is not a part of this ability. The following preliminary experiment was carried out in order to investigate whether subjects could be trained to use a known pressure, and if so, whether the fact that the choice of pressure was arbitrary would affect reproducibility.

The subject was given 2 min. in which to practise reproducing a pressure of 10 lb. by pressing with the thumb on a cork of diameter 2 cm. placed on a Post Office parcels balance. The subject was then asked to reproduce this pressure twenty times at intervals of 20 sec. without seeing the dial. The experimenter gave the signal 3 sec. before the subject began and took the reading on the dial of the balance at a signal given by the subject.

The subjects did the test three times each, then repeated the test 20 hr. later, omitting the practice period. They were shown their results and immediately did another repeat, again without further practice. A third subject was first asked to reproduce any pressure she liked ten times without looking at the dial. This she repeated until she had done six such tests, she was then given the other test to do. The results of all the tests are given in Table 1.

Table 1

Subject		Reproduced pressure in lb. in test number					
		1	2	3	4	5	6
		(*a*) No standard pressure					
3	Mean	9·1	8·5	8·9	8·8	5·9	5·8
	Mean deviation	0·4	0·5	0·7	0·4	0·6	0·3
		(*b*) Standard pressure 10 lb.					
3	Mean	10·3	10·6	—	—	—	—
	Mean deviation	0·7	0·3	—	—	—	—
1	Mean	13·3	10·2	9·8	7·5*	8·1*	—
	Mean deviation	0·8	1·2	0·8	0·7	0·8	—
2	Mean	13·1	12·3	10·4	11·9*	9·4*	—
	Mean deviation	1·1	0·7	0·7	0·8	0·4	—

* No practice period.

These results indicate that subjects can be trained to reproduce a given pressure with sufficient accuracy to justify us in making our objective measurements at this pressure. Moreover, the one subject who also carried out the test using her own standard pressure showed no appreciable difference in accuracy of reproduction when asked to do the test with a standard pressure arbitrarily fixed at 10 lb. These tests would, however, have to be carried out by every subject who was asked to make any judgements using an arbitrarily chosen pressure, to make sure that the results hold for everyone. This in itself would be a somewhat lengthy procedure and it would not even then be known whether this ability acquired with a Post Office balance and a cork could be transferred to the test situation. The subjects may, for instance, have judged by the extent of the movement required rather than by sensations of tension and pressure. The effect of this additional task on the subjects' accuracy of judgement, mental attitude, etc., as distinct from their ability to manipulate the materials successfully, would also have to be investigated. In fact, although the evidence is sufficient to indicate that this is a method which might profitably be pursued further, a whole programme of research would have to be carried out on subsidiary investigations before any confidence could be placed in results obtained by the use of such a method. Moreover, it would still involve measuring abilities other than those necessitated in making judgements of the required character.

V. A METHOD OF CALCULATION FROM SELF-CONSISTENCY DATA

In view of all the difficulties attending the methods described above, it seemed worth while, especially since work on the problems of subjective judgement of firmness is still at such an early stage, to attempt to calculate the threshold from the data already obtained in the earlier experiment on self-consistency (2), even though, admittedly, this would give us information only as to the order of magnitude of the threshold, one of the above more cumbersome methods being necessary for more accurate information.

The data available were the rank orders assigned by nineteen subjects on twenty-five different occasions to eight samples of a specially prepared synthetic material which varied in both spring and firmness. When the experiment was completed, the samples were cut up and objective measurements made under conditions which were as near as possible similar to those used by an average subject. Some of the samples appeared to change slightly during the course of the experiment and, when due allowance was made for this, it appeared that the rank order derived from the means of all the subjective judgements agreed very well with the order of total deformation obtained in the objective tests.

Since the samples changed slightly during the experiment and since also the rank order even for total deformation would depend upon the conditions of testing or handling, the 'objective' differences upon which a subject based any individual ranking would depend upon the time at which the test was done and upon the subject who did it; so that in order to work out a threshold, some way must be found of calculating the objective differences for each person for each half of the experiment taken separately. As a tentative way of doing this, it was assumed that the differences between the mean subjective ranks for each subject for each half of the test were proportional to the differences between the mean total deformations produced by that subject for that half of the test; that is, that in general the subjects' judgements were correct. It might equally have been assumed, remembering Fechner's work, that the subjective differences would correspond with the differences between the logarithms of the total deformation, but for the sake of simplicity the first assumption was made. It was also assumed that the hardest and the softest samples did not change appreciably either with time or with method of handling. Since all subjects agreed as to which were the hardest and the softest and since the mean ranks for these two samples did not change perceptibly during the test, this seems a not altogether unfair assumption to make. Having thus fixed the maximum objective difference, the 'objective' difference for any given pair for any given subject for half the test was taken as a proportion of this, the proportion being determined by the ratio of the difference in the mean subjective ranks of the two samples to the difference between the mean subjective ranks of the hardest and the softest samples for that subject for that half of the test. For example, if eight samples have mean subjective ranks, a, b, c, d, e, f, g, h, and if the hardest (a) and the softest (h) give total deformations A and H respectively, then the objective difference $D-E$ may be calculated as follows:

$$\frac{D-E}{d-e}=\frac{A-H}{a-h} \quad \text{or} \quad D-E=\frac{(A-H)\,(d-e)}{a-h}.$$

This was then taken as a percentage of the deformation for the hardest sample. Now clearly, for those samples which were confused with only one other, the difference in mean rank would be determined solely by the number of times that one was judged greater than the other and not by the *extent* to which it was judged greater, so that with this method of calculating, the 'objective difference' and the percentage correct answers would be different expressions of the same data. For this reason the only pairs selected for analysis were those in which at least one of the pair had been confused with a third sample so that the two mean ranks depended not only upon the number of times one was judged greater than the other, but also upon the extent to which one was judged greater or less than the other. Any sample which was not confused with any others was omitted from the analysis since there was then no measure of the size of the interval between it and adjacent samples. Similarly, if the data tended to fall into two or more groups which were not confused with each other, pairs made up of samples from different groups were not used. Samples which showed a definite drift from one position to another were also omitted.

VI. COMPARISON OF 'MIXED' AND 'SIMPLE' THRESHOLDS

As many pairs of samples as possible were selected in this way from the records of all the subjects. The data were then grouped conveniently and means were calculated. The results are plotted in Fig. 1*a*. Data obtained from pairs that were confused only with each other are also given and it will be seen that the points fall on a straight line. The results obtained from the other pairs form a smooth curve differing markedly from this line, showing that some quite different function is being measured. Moreover, this curve is similar in shape to the curves obtained for the viscosity and modulus thresholds, except that there is a marked tendency to flatten out near the 50 % correct level. This is probably due to the fact that the smaller the differences, the more the relative mean positions of the two samples will tend to be influenced by the number of times one is judged greater than the other and hence the nearer the curve will approach to the straight line obtained for pairs confused only with each other, which cuts the zero difference line at 50 % correct. For this reason data on small differences are considered definitely unreliable and differences below 10 % have been excluded from the calculations.

If, neglecting these points, we plot the logarithm of the percentage difference (log Δ) against percentage correct answers, as was done for the viscosity and elastic modulus thresholds, we get a fairly good straight line (Fig. 1*b*), having a slope significantly lower than that for rubber ($P \leqslant 0{\cdot}01$)[1] and only slightly lower than that for bitumen ($P > 0{\cdot}05$) which it crosses at the 40 % level. This means that the threshold for such a mixed system is barely distinguishable from that for a true fluid. In view of the difficulties of handling the materials and maintaining a stable criterion of firmness in the face of its varying dimensions this threshold is surprisingly low, though it must be remembered that having assumed in the first place that the subjects are in general correct, this method of calculation would tend, if anything, to give an artificially low threshold.

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[1] Lines were fitted by the Method of Least Squares and the sampling errors of the ordinate of the centroid and of the slope were estimated on the assumption that the experimental errors of single points were predominantly in the ordinates: this assumption is probably not entirely justified, but it was considered that the data were hardly sufficient to warrant a more detailed apportionment of the errors between ordinate and abscissa.

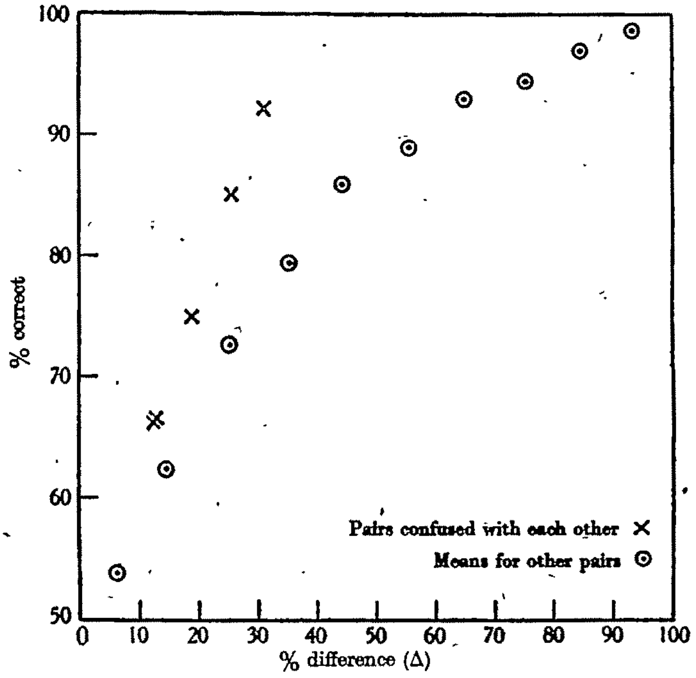
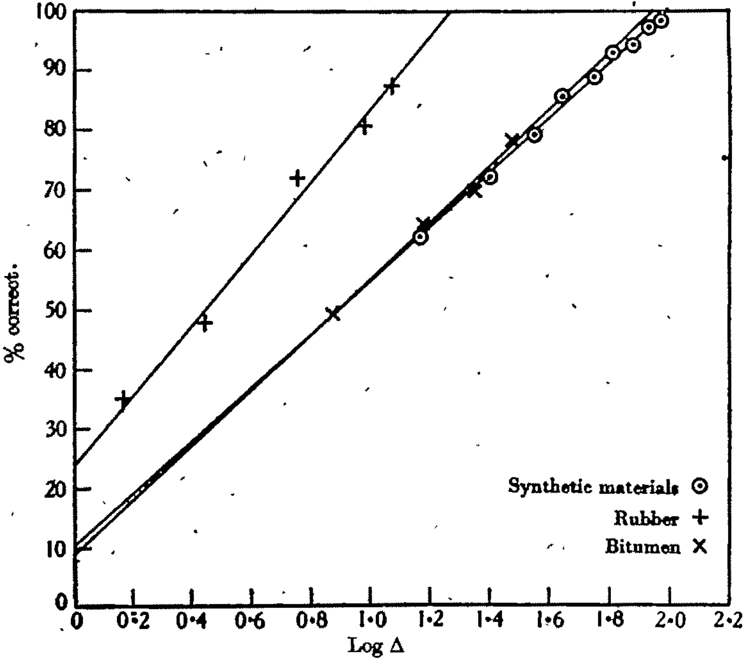

100
90
80
70
60
50
% correct
0
10
20
30
40
50
60
70
80
90
100
% difference (Δ)
Pairs confused with each other ×
Means for other pairs ⊙

Fig. 1 *a*.


100
90
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30
20
10
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% correct.
0
0·2
0·4
0·6
0·8
1·0
1·2
1·4
1·6
1·8
2·0
2·2
Log Δ
Synthetic materials ⊙
Rubber +
Bitumen ×

Fig. 1 *b*.

VII. COMPARISON OF THRESHOLD CURVES FOR ONE CHEESE-MAKER FOR CHEESE AND FOR OTHER MATERIALS

One of the technologist subjects who did the tests with the bitumens, rubbers and synthetic materials, also gave judgements over many months on cheese curd at a certain stage (the pitching stage) in the manufacture of Cheddar and Cheshire cheese. Several cheeses are made each day under the supervision of this expert, who felt each curd just before it was 'pitched' and gave an opinion as to whether it was normal, slightly soft, very soft, slightly firm, very firm, etc., for this stage of manufacture. Altogether she used about eleven such categories of judgement. In this way a rank order could be obtained for the cheese made on any one day. The percentage difference in firmness between each curd and every other curd made that day was calculated from data obtained from an objective test described by Scott Blair & Coppen (3) and the differential threshold curve was plotted.

Cheese curd at the pitching stage is a somewhat unusual material in that it consists of tiny particles from about 25 to 500 mm.^{3} in volume, suspended in whey. When judging for firmness the experts both feel the impact of the particles while stirring in the whey and also squeeze a handful which has been removed from the whey. This particular expert apparently used mainly the latter method. In spite of this difference from the other materials it is of interest to compare this threshold curve with the other curves for the same subject, since she might be expected to give a better performance in the field in which she was especially trained. The results are given in Fig. 2.

Unfortunately this subject's curve for the bitumen experiment is too irregular to be included. The general curves for all subjects are included for comparison.

It will be seen from Fig. 2 that the curve for cheese curd shows a marked curvature, but that nevertheless this subject's performance when dealing with her own material was throughout superior to her performance when dealing with the synthetic cheese-like materials and superior to the average performance for both the true fluids and the synthetic materials. Moreover, for differences above 10 % her performance is actually superior to her own performance on the elastic solids, though still inferior to the average for these materials.

In view of the fact that cheese curd shows properties very similar to those of the synthetic materials, the relative position of the two curves is very striking and seems to afford definite evidence of a superior ability resulting from training. It must, however, be remembered that 'springiness' and firmness are definitely correlated in cheese curd but not in the synthetic materials. This would tend to make judgement relatively easier when dealing with the former material, since for the small differences there would be little difference in 'spring', thus avoiding the dimensional difficulty; and for the larger differences there might be a sufficiently big difference in 'spring' for the subject actually to judge by this difference in quality as well as, or instead of, the difference in firmness. These considerations would account not only for the relative positions of the curves for the synthetic materials and for cheese curd but also for the marked curvature of the latter curve. Added to this, there is probably some correlation between the size of particle and firmness, especially at the extremes, the fine cut curds usually being somewhat firmer objectively and feeling still firmer than the objective test would suggest, thus again exaggerating the great differences.

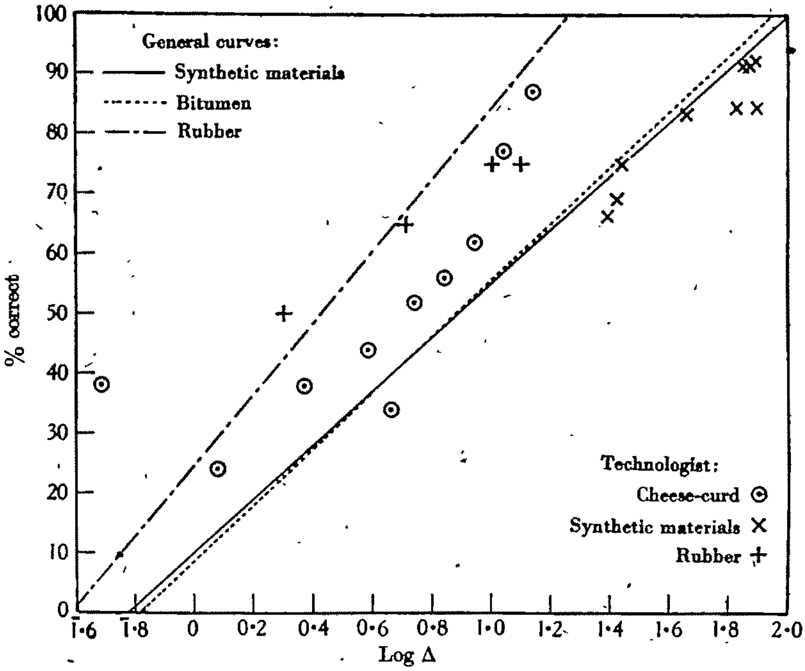


Fig. 2.

It appears that in general the slopes of the threshold curves for this subject are considerably lower than the slopes of the general curves. This suggested that the slope might be characteristic of the subject or of the group for all materials and further plottings for other subjects were done in order to investigate this possibility.

VIII. COMPARISON OF DIFFERENT GROUPS OF SUBJECTS

The data from individual subjects were unfortunately very irregular but where comparison was possible there seemed to be no consistency of slope. The data for groups of subjects having similar training are, however, quite interesting.

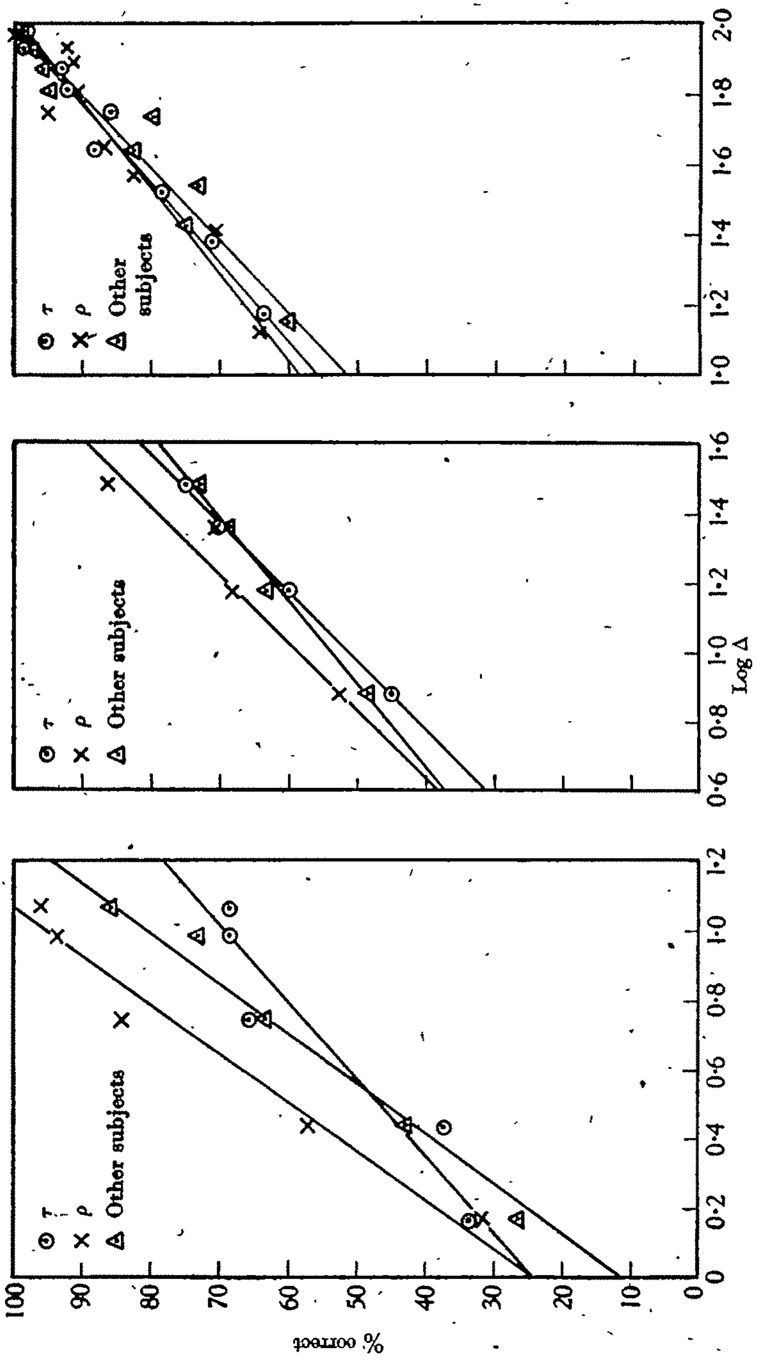
In the previous experiments the two most interesting groups were the expert cheese-makers (τ-group) who are trained in judging for firmness and the routine analysts (ρ-group) who although untrained were almost invariably superior.

The data for these two groups and for the remainder of the subjects were plotted separately for each of the three materials used and the best straight lines were fitted. The results are given in Fig. 3.

The superiority of the ρ-group is again demonstrated in these figures. In every case where there is a significant difference in level between two lines, the ρ-group is involved and is invariably superior. Thus with the rubber cylinders the ρ-group was superior to the rest ($P < 0{\cdot}01$); with the bitumens ρ was superior to τ ($P < 0{\cdot}05$) and probably superior to the rest (P just $> 0{\cdot}05$); for the synthetic materials ρ was superior to the rest ($P < 0{\cdot}05$).

As regards differences in slope, these were disappointing, no significant differences being found either for the bitumens or for the synthetic materials. For the rubber, however, the τ curve was lower in slope than either the ρ curve ($P < 0{\cdot}05$) or the rest (P just $> 0{\cdot}05$). The reasons for a difference in slope are hard to determine but it seems likely that they could be caused by differences in the influence of chance factors on the subjects, though if this is the case it seems surprising that chance factors should apparently have played a more important part in the task which is, of the three, the simplest from the point of view of physical theory and is also reported to be the easiest to perform. It may be, of course, that this very simplicity gives rise to boredom and it has already been suggested (1,2) that the expert technologist cheese-makers are more susceptible to boredom than are other categories.

It appears then that, as in previous experiments and also in the previous analysis of these data, the technologists are in no way superior to the average in judging the firmness of the materials used. It still remains possible, however, that this is merely due to failure to transfer



(*a*) Rubber.

(*b*) Bitumen.

(*c*) Synthetic materials.

Fig. 3.

ability and that they may show superiority when dealing with their own materials. This seems especially likely considering the results of the one expert cheese-maker whose threshold for cheese curd was measured. One might have expected, though, that the ability to reproduce test conditions and to maintain a stable mental criterion of firmness would be transferable, whereas no such transference is shown in this experiment. On the other hand, ability to select *suitable* test conditions acquired when dealing with cheese-curd might very well not be transferable to a new material and it is possible that this factor plays its part in the superiority shown by the single cheese-maker tested on her own materials, though the alternative explanations seem more likely.

IX. GENERAL DISCUSSION

The results obtained by this method of calculation serve to indicate the importance of the ranking method as a way of measuring a differential threshold where judgement is slow. It has the great advantage for this type of work that it is far quicker than any other method. By one ranking of eight samples, a subject has in effect given judgements on twenty-eight pairs, and the average time taken to do this for the samples used in this experiment was less than 5 min., whereas most subjects took about 20 min. in the modulus experiment and about 40 min. in the viscosity experiment, to give the same number of judgements by a modification of the method of paired comparisons.

A more important question which arises when considering the whole of this and similar work (cf. Katz (4) on flour-doughs and Binns (5) on softness of wool-tops) is the question of the true nature of the subjective concept 'firmness'.

Sullivan (6) has approached this question by attempting first to analyse by means of introspections, then to synthesize the cutaneous perception of 'softness' and 'hardness'. Wooden and metal cylinders, wads of cotton, etc. were pressed on the subjects' hands and it was found that hardness and softness depend on:

(1) The pattern of pressures in the area stimulated;
(2) The sharpness of the boundary line;
(3) Temperature.

Neither intensity of pressure nor lateral movement seemed to play any appreciable part. She came to a somewhat similar conclusion in her experiments on the cutaneous perception of 'liquidity', 'semi-liquidity' and 'solidity' (7), in that pressure and temperature were found to be the dominant factors, time effects playing a negligible part. This is very surprising, for the physicist and probably most laymen would define 'firmness' as some function of the relationship between the pressure applied and the amount or the rate of the deformation or flow produced; which leads to the conclusion that the cutaneous senses can play little or no part in the judgement of 'firmness' in this sense and in fact may well provide a serious source of error. This, indeed, was shown by Sullivan herself for, in comparing wooden cylinders, she got marked reversals of judgement with differences in the roughness of the surfaces or with temperature differences. Unfortunately the whole of this work was somewhat marred by the, at that time, almost inevitable lack of rheological knowledge, with consequent confusion and ambiguity of terminology. Nevertheless, these factors emphasized by Sullivan may have to be taken into account in a full study of the subjective concept of 'firmness', though it must be remembered that they are not part of what a subject consciously means or understands by the word 'firmness'; rather they are properties which under suitable conditions may arouse an illusory perception of 'firmness' or 'softness'.

The appropriate stimuli for a perception of what is consciously meant by 'firmness' are of necessity many and varied and include lateral movement over the skin, pressure on the skin and underlying tissues and tension and movement in muscles and joints. Considering the problem from the purely psychological point of view, it is most probable that 'firmness' is an 'emergent character' resulting from such simple sensory data in a manner such as is decribed in Watt's *The Sensory Basis of Knowledge* (8). For example, he says, "When two pressure contacts are set some distance apart on the skin...we notice the presence not only of the two spots of sensation, but of another sensory datum, namely, the *distance* between them". This distance is not a property inherent in the spots although it is inevitably bound to them, it seems, in other words, that it is part of the Gestalt but not part of the stimuli which form the basis of the Gestalt. Similarly, it may be that 'firmness' is an 'emergent character' resulting from certain sensations of pressure and movement. Certainly something of the sort must be true, though how far one would be justified in regarding 'firmness' solely as the 'emergent character', that is, as that which is a property of the Gestalt but not of the parts or how far one should regard it as the *whole* Gestalt, it is impossible at this stage to say.

In either case one would have to assume that the basis of the Gestalt, or the conditions for the emergence of the new character, are not always the same, that is, that the presence or absence of another factor, time, does not affect it. For, as has been previously shown, the term 'firmness' is applied with equal ease to true fluids where it can only be an expression of the proportionality between stress and *rate* of strain and to true solids where it can only express the proportionality between stress and *amount* of strain independent of time. Moreover, the data from the cutaneous senses alone can give rise to an experience of 'firmness' or 'softness', or they can be eliminated for comparative purposes without preventing comparisons for firmness.

From the purely physical point of view the problem is still more complex. For the simple systems, true fluids and true solids, the position is clear enough. Firmness can refer only to the compression modulus in the latter case, and to viscosity in the former, but for intermediate materials there is an almost infinite series of viscosities for different stresses, another series for different strains and in all probability there are many different compression moduli also. In view of these difficulties, in the present experiments, measurements of deformation and of recovery were made under conditions of stress and strain as near the estimated average used by our subjects as could conveniently be done. It was unfortunately impossible to use as short a time as used by the subjects, and 2 sec. was used which is at least twice the average subjective time. The various measurements were then compared with the mean subjective ranks for all subjects in order to see which measurement correlated most nearly with 'firmness' as understood by these subjects. The best agreement was found for the inverse of the total deformation under a standard stress acting for the given time (2 sec.).

This is interesting in view of the equation proposed by Nutting (9) and later independently by Scott Blair & Coppen (1) giving the following expression for firmness (ψ)[1]:

$$\psi = S\sigma^{-1}t^{k},$$

where S = shearing stress, σ = total strain, t = time, k is a parameter between 0 and 1.

For unit time (1 sec. on this occasion) $\psi = S\sigma^{-1}$, or for a given stress firmness is inversely proportional to total deformation. Now in this case t was 2 sec. and yet subjective firmness was apparently proportional to total deformation which suggests that unit time for these subjects was of the order of 2 sec. even though the actual time of compression was less than this for most subjects. This corroborates the findings of another experiment (10) in which the materials judged were a true fluid and a true solid, i.e. where the difference in k was maximal. In this experiment it was found that for a compression time of 1 sec. a true solid would be judged of the same firmness as a true fluid of viscosity numerically three times the modulus of the solid, in other words the ratio of subjective to objective time unit appeared to be of the order of 3, or, taking the usual physical unit of 1 sec., the subjective unit was found to be 3 sec. It appears then that in judging firmness the subjective unit of time is not the same as the physical and is not even a constant but depends upon the difference in k for the two samples, decreasing as this difference decreases. This matter is being further investigated.

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[1] There is something to be said for defining the physical concept 'firmness' as $\log_{10}\psi$, but the matter is not yet fully settled.

X. SUMMARY

1. The difficulties from both the physical and psychological points of view of comparing for firmness materials which vary also in 'spring' are discussed, and methods of overcoming these difficulties suggested and criticized.

2. Investigation by measuring self-consistency of judgement is useful for comparing individuals but gives no direct data concerning objective differences from which a threshold may be calculated.

3. The use of artificial self-recording materials is recommended but the effects of necessary differences from normal materials would have to be investigated.

4. The use of a known pressure is suggested and a preliminary experiment described in which ability to reproduce a pressure chosen by the experimenter was measured and this ability compared with reproducibility when the pressure was chosen by the subject. Results indicated that this is a promising line of approach but further investigations are necessary before it can be used.

5. A method is described by which, if certain reasonable assumptions are made, the necessary data for a threshold curve can be estimated from self-consistency data already obtained.

6. The threshold curve so derived was found to be almost indistinguishable from that for the firmness or viscosity of true fluids.

7. The threshold curve for cheese curd for a skilled cheese-maker is compared with other threshold curves and found to be lower than those for true fluids and for synthetic cheese-like materials. Possible reasons for this are discussed.

8. A logarithmic plotting of the curves for different groups of subjects confirmed the superiority, shown as threshold level, of the routine-analysts. Differences in slope were insignificant, except in one case where the technologist group showed a lower slope, lending added weight to previous suggestions concerning the mental attitude of this group.

9. The meaning and nature of the subjective concept 'firmness' are discussed.

The author wishes to thank Dr P. White for his help with the statistical treatment of the data, the late Mr F. Johnson Dix for his encouragement and many valuable ideas including specifically those regarding the design of artificial materials, and Dr Scott Blair for his co-operation on the experimental side and continued help and advice.

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THE USE OF INTERACTIONS IN ANALYSING PSYCHOLOGICAL DATA

By WILLIAM STEPHENS TAYLOR

I. *Introduction to the use of interactions* (pp. 248–249).
II. *The method of calculating interactions* (pp. 249–251).
III. *The use of interactions with psychological data* (pp. 251–255).
IV. *A comparison of interactions and correlations* (pp. 255–256).
V. *The nature of the difference between interactions and correlations* (pp. 256–258).
VI. *Conclusion* (p. 258).
References (p. 258).

I. INTRODUCTION TO THE USE OF INTERACTIONS

THE purpose of this paper is to urge the importance of calculating interactions for all quantitative psychological data ordinarily analysed by correlational methods. The statistic for interactions is that obtained in the calculation of the Analysis of Variance (1, 2).

The value of this statistic for the analysis of psychological data appears in two main ways:

(*a*) It supplements the calculation of correlations by giving additional information about the interrelations of series of observations, and suggests that the correlation coefficient, by itself, provides an inadequate basis for a psychological analysis which is supposed to be in any sense exhaustive.

(*b*) More particularly it gives specific information about the way in which two or more abilities, as measured, for instance, by particular tests, co-operate to produce the standard of achievement with which the individual is credited. In this way it gives an important type of information about the interrelationships of abilities which cannot be obtained adequately from the correlation coefficient.

The use of this statistic as a supplement to correlations requires little extra labour, as the sums of squares of deviations about the mean are already given in the calculation for correlations. When used independently the calculation of interactions requires less labour than the calculations of correlations, as it does not involve the use of sums of products.

The meaning to be drawn from the statistic for interactions may be illustrated by the following example ((1), Chap. VII, Table 46):

	Basal dressing	Sulphate
Kerr's Pink	8·88	10 43
Nithsdale	8·38	8·36

The statistic for interactions would disappear if the entries were strictly in proportion, that is, if 8·88 was to 8·38 as 10·43 was to 8·36. The statistic for interactions measures the extent to which the entries deviate from this strictly proportional form. The meaning of the disproportion is derived from the nature of the particular experiment. In this instance it would indicate that the addition of a sulphate dressing was effective in increasing production only in the presence of a certain species of potato plant. The sulphate dressing was not operative in virtue of its own characteristics alone, but in virtue of a conjunction between its own characteristics and another specified factor. The co-operative action of sulphate dressing and certain plant characteristics produced an effect which could not be explained as a merely additive effect of the contributions each makes independently.

In the calculation of disproportionate entries by interactions, a reliable statistical measure of significance is obtained.

II. THE METHOD OF CALCULATING INTERACTIONS

The following hypothetical example will indicate the nature of the calculation involved in estimating the statistic for interactions.

The principle of the analysis is given in Fisher's equation

$$S(x-\bar{x})^2 = S\{n_p(\bar{x}_p-\bar{x})^2\} + SS(x-\bar{x}_p)^2.$$

When the mean values of the different groups into which the scores can be divided do not differ significantly among themselves, the sum of squares of deviations about the grand mean does not differ significantly from the sum of squares of the deviations of the scores about their group means, i.e. the second term of the equation virtually drops out. Where the group values do differ among themselves, however, this is not true. In this case the analysis of variance divides up the total sum of squares of the deviations of all scores about the grand mean into the portion attributable to the differences between the mean values of specific groups, the portion attributable to the disproportionate relations between the mean values of the groups (i.e. interactions) irrespective of absolute differences, and the portion attributable to deviations of the scores within each group about its group mean. In normal procedure the difference between the means of any two groups is tested for significance against the deviations of the observations within the groups from their own group means. The same procedure is followed here. The sum of squares attributable to deviations of the observations within groups from their group means provides the value against which the other estimates are

	Test I			Test II				
College A	21			24			College A totals:	
	19			15				
	25			13				
	24	Sx	230	24	Sx	210	Sx	440
	20	Mean	23	30	Mean	21	Mean	22
	25	$(Sx)^2$	52900	18	$(Sx)^2$	44100	$(Sx)^2$	193600
	30	$S(x^2)$	5476	25	$S(x^2)$	4676	$S(x^2)$	10152
	30			16				
	18			25				
	18			20				
College B	22			24			College B totals:	
	24			32				
	26			18				
	23	Sx	300	12	Sx	200	Sx	500
	37	Mean	30	20	Mean	20	Mean	25
	32	$(Sx)^2$	90000	15	$(Sx)^2$	40000	$(Sx)^2$	250000
	40	$S(x^2)$	9344	30	$S(x^2)$	4428	$S(x^2)$	13772
	35			21				
	30			15				
	31			13				
		Total Test I			Total Test II		Grand total	
		Sx	530		Sx	410	Sx	940
		Mean	26·5		Mean	20·5	Mean	23·5
		$(Sx)^2$	280900		$(Sx)^2$	168100	$(Sx)^2$	883600
		$S(x^2)$	14820		$S(x^2)$	9104	$S(x^2)$	23924

Total s.s. of deviations from the grand mean

$$=S(x-\bar{x})^2=S(x^2)-\frac{(Sx)^2}{N}=23924-\frac{883600}{40}=1834.$$

Total s.s. of deviations for group differences and interactions

$$=\frac{(Sx)^2+(Sx)^2+(Sx)^2+(Sx)^2}{\text{No. in each group}}-\text{c.f.}^*$$

$$=\frac{52900+90000+44100+40000}{10}-\text{c.f.}= \qquad 610$$

s.s. for college differences $=\dfrac{(Sx)^2+(Sx)^2}{\text{No. in group}}-\text{c.f.}$

$$=\frac{193600+250000}{20}-\text{c.f.}= \qquad 90$$

s.s. for test differences $=\dfrac{(Sx)^2+(Sx)^2}{\text{No. in group}}-\text{c.f.}$

$$=\frac{280900+168100}{20}-\text{c.f.}= \qquad 360$$

s.s. for interactions (obtained by subtracting the specific group differences from the total attributable to group differences and interactions) 160

—— 610

s.s. of deviations within groups $=1834-610$ $=$ 1224 tested for significance. When estimating the sums of squares of deviations attributable to group differences, any groups or combinations of groups may be used, with the single proviso that in making any comparisons, all the observations belonging to those groups must be used in the calculation. As the example on p. 250 shows, the various groups are treated separately in estimating the total sum of squares attributable to group differences and interactions, and are then taken in various combinations to isolate particular group differences. The various sums of squares are reduced to a statistic 'z' in order to test them for significance. This is done by calculating $\frac{1}{2}\log_e$ of the mean square of the various sums of squares obtained, and subtracting the log of the 'within groups' value from the logs of other values.

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* c.f. (correction factor) for sums of squares is always $\frac{(Sx)^2}{N}$ as in first line of calculation

		Degree of	mean				
	S.S.	Freedom	Square	$\frac{1}{2}\log_e$	'z'	P 0·05*	P 0·01*
Between college groups	90	1	90	2·2499	0·4867	0·7141	1·0116
Between test groups	360	1	360	2·9431	1·1799	0·7141	1·0116
Interactions	160	1	160	2·5376	0·7744	0·7141	1·0116
Within groups	1224	36	34	1·7632	—	—	—

* These values are given in Fisher's tables (1).

III. THE USE OF INTERACTIONS WITH PSYCHOLOGICAL DATA

The statistic for interactions may be used with psychological data, independently of the calculation of correlations, to give the type of information described above. The following is an example:

	Test of eductive ability	Logical memory test
Test taken in native language	21·6	19·4
Test taken in acquired language	20·2	13·8

The values entered are mean scores for groups of forty-one and forty-five students respectively. It is obvious that the language in which the test is taken has a much more significant influence in conjunction with the ability required by the logical memory test than in conjunction with the ability required by the test of eductive ability. The influence of the language difference becomes operative only in conjunction with another specified factor.

In a situation such as this the number of tests used may be increased indefinitely, and those test abilities isolated in conjunction with which the difference of language becomes significantly effective. Similarly, any differences or groups of differences which the experimental procedure requires may be used in place of the language difference of this example.

There are many of these which are of particular interest for social and educational psychology, such differences as those of sex, age, educational achievement, race, nationality, religion, economic status, family type. The calculation of interactions is of particular value in isolating the characteristics in conjunction with which the occurrence of such differences becomes socially and psychologically important.

By a slight modification in experimental design, the calculation of interactions can be used to give information about the interrelations of mental abilities which is of importance to theoretic psychology. It can indicate, (1) those abilities which tend normally, with given types of problems, to be used in conjunction, and (2) those abilities which, when occurring in conjunction, produce an increase of effectiveness which cannot be explained either by the intrinsic features of either or both of them taken in isolation or by merely additive effects of their contributions; thus such abilities are distinguished from others whose occurrence in conjunction has a neutral, or merely-additive effect. One need only substitute for language differences, in the above example, differences in the degree to which two or more groups possess an ability as measured by a particular test, and calculate the interactions with abilities as measured by other tests.

In the following example two groups are used, of which one had a relatively high degree of eductive ability as measured by the Analogies test, and the other a relatively low degree of eductive ability as measured by the same test. The high eductive group is composed of students having the upper third of scores on the Analogies test, and the low eductive group of students having the lower third of scores. The figures are taken from the scores of students in India on a battery of tests. It will be noticed that the standard of achievement, as indicated by the mean values, varies considerably from test to test. In the calculation of interactions the sums of squares of deviations from the mean attribute to such differences in standard of achievement are partialled out before the value for interactions is calculated, so that the latter remains a statistic based only on the proportional relations of the values, however these may differ in absolute value from test to test. Table 1 gives the mean scores for the two groups on eight tests, and Table 2 gives the calculated values of the interactions when the tests are taken by pairs in all possible combinations (p. 254). The entry in any row and column indicates the value of the interaction between the eductive groups and the tests indicated by that row and column. For example, the value given in row 3 column 5, which we may for convenience designate as I_{35}, is the value of the interaction between the eductive groups on the one hand, and the abilities required by the tests of Absurdities and Cancellation A on the other. By reference to Table 1 we see that the values from which this interaction is derived may be presented, as in previous illustrations, in the following form:

	Absurdities	Cancellation A
High eductive ability	63·1	39·5
Low eductive ability	46·0	42·0

One may interpret the interaction as indicating that the possession of high eductive ability is a positive help in the response to the Absurdities test to a much greater extent than in the response to the Cancellation A test. In terms previously used, the conjunction of high eductive ability with the other abilities required in the solution of the Absurdities test produces a much greater measure of achievement than does the conjunction of high eductive ability with the other abilities required in the Cancellation A test. The differences in achievement cannot be explained by differences between the two levels of eductive ability alone, nor by differences between the test problems alone, nor by the merely additive effects of the two, but must be explained as an enhanced effect due to the conjunction of high eductive ability with the demands made by a particular test problem. Similarly, in the entry I_{78}, the statistic for interactions indicates that the various differences in levels of achievement cannot be explained either by differences in eductive ability, taken alone, or by differences in the demands of the two tests, taken alone, or by a merely additive effect of the two, but must be attributed to an enhanced level of achievement produced in particular by the conjunction of high eductive ability and the other abilities required to solve the Logical Memory test, but not found to the same extent when high eductive ability occurs in conjunction with the other abilities required by the Retention test.

The tests used, and the numbers by which they are referred to, are as follows:

1. Jumbled sentences.
2. Number series.
3. Absurdities.
4. Completion.
5. Cancellation A.
6. Cancellation P, R, S, T.
7. Retention.
8. Logical memory.
9. Analogies.

Table 1. *Mean scores*

Test...	1	2	3	4	5	6	7	8
High eductive ability	83·9	76·7	63·1	37·8	39·5	42·9	75·4	32·9
Low eductive ability	76·3	63·1	46·0	27·2	42·0	44·6	76·1	20·7

Table 2. *Calculated values of the interactions* (I_{ij})

	1	2	3	4	8	5	6	7
1		0·2803	0·2803	—	—	0·5954	0·4462	0·2859
2	0·2803		—	—	—	0·7599*	0·6750	0·5789
3	0·2803	—		—	0·3272	1·1673†	1·0834†	0·9741*
4	—	—	—		—	0·9877†	0·8476*	0·6825
8	—	—	0·3272	—		1·0090†	0·9599*	0·8028*
5	0·5954	0·7599*	1·1673†	0·9877†	1·0090†		—	—
6	0·4462	0·6750	1·0834†	0·8476*	0·9599*	—		—
7	0·2859	0·5789	0·9741*	0·6825	0·8028*	—	—	

* Significant on the 0·05 level.

† Significant on the 0·01 level.

— Where the entry is left blank, the mean square for interaction is less than that for error.

The tests have been rearranged in order, so as to bring out more clearly the points noted in the text.

An examination of Table 2 suggests several points:

(1) The presence or absence of high eductive ability affects achievement in tests 1, 2, 3, 4, and 8 in ways so nearly the same that differences between them are completely insignificant. Eductive ability operates in almost exactly the same way when it functions in conjunction with any of the other particular abilities required by these tests.

(2) Similarly eductive ability operates in almost exactly the same way when it functions in conjunction with any of the particular abilities required by tests 5, 6 and 7.

(3) The way in which eductive ability functions in conjunction with the abilities required by the first group of tests is, however, radically different from the way it functions in conjunction with abilities required by the second group of tests. Their occurrence in conjunction tends to enhance achievement in the former, but to produce an effect in the latter which is either neutral or detrimental to achievement.

(4) The only significant exception to this general distinction is test 1, as eductive ability appears to operate in conjunction with the abilities required by this test in a way which is not significantly different from that in which it operates in conjunction with the abilities of tests 5, 6 and 7.

(5) I_{35} and I_{36} suggest that test 3 (Absurdities) provides the most striking instance of achievement being enhanced by a co-operation between eductive ability and other particular abilities, while tests 5 and 6 (Cancellations) give the most striking instances of achievement being neutralized by the occurrence in conjunction of eductive and other abilities.

IV. A COMPARISON OF INTERACTIONS AND CORRELATIONS

When the scores of the students, from whom the above two groups were obtained, on the same nine tests, are correlated in the ordinary way, the following Table 3 of Pearson Product Moment Correlation Coefficients is obtained.

Table 3. *Product moment correlation coefficients*

	1	2	3	4	8	5	6	7	9
1		0·3611†	0·3761†	0·1840	0·3800†	0·0422	0·2876*	0·1296	0·3966†
2	0·3611†		0·2025	0·2061	0·2239	−0·0084	0·1994	0·1065	0·4014†
3	0·3761†	0·2025		0·2996*	0·3780†	−0·1479	−0·0120	−0·1962	0·5161†
4	0·1840	0·2061	0·2996*		0·3589†	0·0059	0·3001*	0·0318	0·2850*
8	0·3800†	0·2239	0·3780†	0·3589†		−0·1002	0·1484	−0·0949	0·3326*
5	0·0422	−0·0084	−0·1479	0·0059	−0·1002		0·3374*	0·4333†	−0·0503
6	0·2876*	0·1994	−0·0120	0·3001*	0·1484	0·3374*		0·2003	0·0361
7	0·1296	0·1065	−0·1962	0·0318	−0·0949	0·4333†	0·2003		0·0995
9	0·3966†	0·4014†	0·5161†	0·2850*	0·3326*	−0·0503	0·0361	0·0995	

* Significant on the 0·05 level. † Significant on the 0·01 level.

When this table is compared with Table 2 above, significant correlations are generally found to correspond to insignificant interactions and vice versa. There are, however, a number of striking exceptions which prevent one's assuming that the same psychological interpretation can be drawn regularly from both high correlations and low interactions, or vice versa.

(*a*) There are several instances where both correlations and interactions are insignificant: r_{23} and I_{23}, r_{14} and I_{14}, r_{24} and I_{24}, r_{17} and I_{17}, r_{28} and I_{28}, r_{67} and I_{67}. The insignificant correlations in r_{15}, r_{26}, r_{27}, and r_{47} are also accompanied by corresponding insignificant interactions, although in these cases the interactions approach much nearer significance.

(*b*) There is one case where a significant correlation, r_{46}, is paralleled by an equally significant interaction, I_{46}.

(*c*) There are some correlations which are very similar in size, but are paralleled by interactions which differ considerably in size, e.g. r_{14} and r_{26}, r_{17} and r_{68}, r_{34} and r_{46}.

One further detail throws light on differences in interpretation. From the correlations r_{93} and r_{94} the psychologist would normally conclude that the ability required by test 9 could be identified to a much greater extent with the ability required by test 3 than with that required by test 4. The interaction I_{34} however, between the ability required by test 9 on the one hand and those required by tests 3 and 4 on the other hand, is so completely insignificant that the mean square for interactions is actually less than that for error. One must conclude that the way in which the ability required by test 9 operates when it occurs in conjunction with test 3 conditions is indistinguishable from the way it operates when in conjunction with test 4 conditions. It is difficult to reconcile this with the inference drawn from the correlation coefficient.

V. THE NATURE OF THE DIFFERENCE BETWEEN INTERACTIONS AND CORRELATIONS

These differences in interpretation are due to the fact that the statistics for correlations and for interactions measure different features of a complex table of scores. The statistic for correlations is governed primarily by a correspondence of rank order between two sets of scores, and differences either in the standard of achievement or in the spread of the two sets of scores between upper and lower extremes are of relatively minor importance. On the other hand, it is just these features of the table of scores about which the statistic for interactions gives information, and questions of rank order are of minor importance only. A significant interaction indicates that, due to the influence of a factor which may be experimentally controlled, the difference between the standards of achievement of two groups in a particular test is disproportionately greater than the difference between their standards of achievement in another test.[1] Using logical terms, we might say that the calculation of correlations is a statistical form of the method of induction by concomitant variations, while the calculation of interactions is a statistical form of induction by the combined method of agreement and difference. Probably no logician would admit that either of them by itself is capable of giving conclusions which are sufficiently reliable to provide a basis for extended conclusions about the nature of mental abilities. The calculation of correlations, at any rate, when used by itself obviously cannot provide a basis for thoroughly dependable inferences regarding mental abilities, since, as we have seen, its results so frequently conflict with those obtained by the equally valid method of interactions. But when used in conjunction, any results supported by both methods may be depended on.[1]

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[1] The statistic for interactions is thus a measure of tetrad differences. A strictly hierarchical table would show no significant interactions. In the method here outlined, however, the statistic is applied to the original data, rather than to correlation coefficients. Interactions and correlations are calculated for the same table of data. The two methods thus supplement and correct each other, as the forms of logical induction are traditionally required to supplement and correct each other before the results of any single method can be considered satisfactory. In ordinary factor analysis, however, the calculation of tetrad differences is applied to correlation coefficients. This appears rather like applying one method of induction to the results obtained by another method, instead of applying them both to a common set of data.

The difference in method can be illustrated in more detail from data already presented. From the mean values given in Table 1 the following may be abstracted:

	Test 4	Test 6	Test 5	Test 8
High eductive ability	37·8	42·9	39·5	32·9
Low eductive ability	27·2	44·6	42·0	20·7

These entries form two 2 × 2 tables which are similar in the proportions which the entries bear to each other. Each table contains one test in which the possession of high eductive ability tends to produce a greatly enhanced level of achievement, and one in which the possession of high or low eductive ability makes no important difference. The values of 'z' for the interactions I_{46} and I_{58} (Table 2) are consequently definitely significant and very similar. The correlation coefficients r_{46} and r_{58} however (Table 3) are very different, one being significantly positive and the other small and negative. These correlation coefficients are based on the use of more data than that used for the interactions, as they are based on the whole table of scores, in the ordinary way, from which the upper and lower thirds were extracted in the calculation of interactions. When the correlation coefficients are calculated for those scores only which were also used in calculating interactions, the coefficients are as follows:

$$r_{46} + 0{\cdot}3283, \quad r_{58} - 0{\cdot}2550.$$

The difference between the coefficients is more striking now than before. Moreover, if the correlation coefficients are calculated separately for the high G and low G groups, the r_{46} coefficients for both groups are found to be positive, averaging a little higher than 0·3283, and the r_{58} coefficients for both groups are found to be negative, averaging slightly less than

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[1] An example of their use in conjunction will be found in my article on 'Some effects of education in an acquired language', in a recent number of the *Brit. J. Educ. Psychol.*

−0·2550. It is obvious that the two statistics are based on different features of the tables of scores. Both are legitimate statistics to use.

Of the two, the statistic for interactions can perhaps be given a more definite meaning in experimental procedure, and its use may consequently be freed from some of the ambiguities which tend to vitiate so many inferences based on correlational evidence alone.

VI. CONCLUSION

To sum up, we have found:

(1) That the statistic for interactions can be calculated for all psychological data which are normally analysed by correlational methods.

(2) That it can be used with the greatest advantage to supplement and correct information obtained by the calculation of correlations.

(3) That it is a valuable tool for estimating the extent to which certain factors, functioning in conjunction with each other, produce effects explicable neither by reference to any or all of them functioning in isolation, nor by a merely additive effect of their respective contributions.

(*a*) It can be used with the greatest advantage to discover those interrelationships of mental abilities by virtue of which they function, in conjunction, in this non-additive way.

(*b*) It can also be used to advantage to isolate those particular characteristics in conjunction with which a given difference of social importance produces significant effects.

The study of how our mental abilities co-operate when applied in conjunction to the solution of a complex problem is of the utmost importance for psychology. As the statistic for interactions is designed to give information on just such problems, and as there is a valid measure for determining its significance, it could be extensively used, with decided advantage, in the analysis of quantitative psychological data.

REFERENCES

(1) FISHER, R. A. (1938). *Statistical Methods for Research Workers.* Edinburgh: Oliver and Boyd.

(2) SNEDECOR, G. W. (1934). *Calculation and Interpretation of Analysis of Variance and Covariance.* Iowa: Ames.

(*Manuscript received* 5 *August* 1941)

NOTE

In Table 1 on p. 140 of the paper by Boris Semeonoff, 'Dark Adaptation during Stimulation with Coloured Light' (this *Journal*, XXXII, Part 3), the heading "in." should read "min."

PUBLICATIONS RECENTLY RECEIVED

The Cambridge Evacuation Survey. Edited by SUSAN ISAACS, with the co-operation of SIBYL CLEMENT BROWN and ROBERT H. THOULESS. Contributions to Modern Education, edited by Susan Isaacs. London: Methuen and Co., Ltd. 1941. Pp. ix + 235. 8*s*. 6*d*. net.

This book is sure of a welcome among psychologists, since it is the first to give a full and detailed psychological study of a group of evacuated children, both those who stayed and those who went back. Out of over 3000 school children evacuated to Cambridge in September 1939, rather over 1000 remained in July 1940. But the main part of the study was confined to 300 children from Tottenham, made during December 1939; 350 children from Islington, made during the spring and early summer of 1940; and 69 Tottenham children and 81 Islington children who returned to London before these dates. Questionnaires relating to the children in Cambridge were sent out to the billeting officers, asking for information about the foster homes, the attitude of the foster parents to the evacuated children, and the behaviour of the child and its parents. These children also wrote essays on "What I like in Cambridge" and "What I miss in Cambridge". Forty children badly adapted to their foster homes were personally interviewed and tested, and compared with a control group of 40 well-adjusted children; and additional data were obtained from other children referred to the Child Guidance Clinic. The families of the returned children were interviewed personally by trained social workers. From the results of the data obtained from the Cambridge children, a useful Memorandum on Practical Recommendations was drawn up and circulated among Public Authorities, etc. This Memorandum, which is given in full, is admirable, and should be studied carefully by everyone interested in evacuation.

The main points that emerged from the survey were that, according to the questionnaire data, only 8·2% of the children were definitely unhappy in their billets, but that, among the Tottenham children, the proportion was much higher among adolescents than among younger children. The presence of siblings in the same billet made for greater happiness; so also did fairly frequent visits from parents. Difference in economic status between parents and foster parents seemed to have little effect on happy adjustment. From the more detailed studies it appeared that misfits could often have been avoided by billeting the more lively children in families with youngish foster parents and other children; and the quieter and more withdrawn children in homes with older foster parents and no other children. The most boisterous and aggressive type of child was always likely to be a problem; and there was a certain number of difficult cases who could only be dealt with by institutional treatment. But there was no evidence of widespread emotional maladjustment arising simply as the result of evacuation, either among the children in Cambridge or those who had returned. In many cases there was an improvement of health. The main factors which caused return were (*a*) anxiety or loneliness of the parents and/or children, (*b*) parents' dissatisfaction with the foster home, (*c*) parents' poverty—cost of billeting, clothes, visits, etc. But there seemed to be little evidence of any general grudge against evacuation as such.

This book is so interesting and so well written and constructed that it seems ungracious to carp at it. Indeed the authors themselves forestall criticism on many points. It is of course obvious that the sample studied was too small and limited in range and variety to give much reliable indication as to the effects of evacuation in general. For this reason, the use of questionnaires and statistical methods seems rather inappropriate. Little reliance can be placed on any except purely factual information obtained from those untrained in answering questionnaires; and too much weight seems to have been given to the answers to the question, "Is the child happy?" But very useful data relating to the child's underlying psychological attitudes and reactions to evacuation might have been obtained by an intensive study such as those which Dr Isaacs has carried out in the past. Here, however, the individual case studies were neither sufficiently numerous nor detailed and 'deep' to give much information of this kind.

Thus the book seems rather to fall between two stools, the sociological and the psychological, when it might more profitably have been confined to either. But it is useless to demand what no doubt the circumstances of the moment made it impossible to provide; and we must welcome the book as it stands.

M. D. V.

Change. Clothes Rationing Survey: An Interim Report prepared by MASS-OBSERVATION. London: No. 1 Bulletin of the Advertising Service Guild. 1941. Pp. vi+85. 1*s.* net.

From the point of view of the Clothing and Advertising trades it was no doubt important to gain an impression as rapidly as possible as to the effects of rationing upon the British public and its 'clothes habits'. Unfortunately it seems to have been a case of "more haste, less speed". The mind of the British public moves too slowly for it to be able to decide, within the space of about a month, what alterations if any it is likely to make in its buying and wearing of clothes. The general impression is that most people were resigned to rationing, and that they did not anticipate much alteration in their manner of dressing, except that they would worry less about smartness and social standards of dressing (this especially among men).

In a brief report such as this it was no doubt impossible to describe in detail exactly how the information was obtained (methods of sampling, interviewing, etc.). But we are told that 300 people in each of three towns, London, Worcester and Bolton, were given a verbal questionnaire, which is quoted in full. The persons questioned were selected with a view to obtaining a representative survey of all classes and types of occupation. Also various supplementary reports were obtained, from "Mass-Observation voluntary workers" and others. The data seem sufficiently reliable to allow the acceptance of the rather negative conclusions of the Report. But the small size and narrow distribution of the samples would necessitate great caution in accepting any positive trends, which might be largely influenced by individual idiosyncrasy.

Nevertheless, the Advertising Service Guild are to be congratulated on its publication of this Report. Perhaps it will see its way in future to commissioning larger and more detailed surveys.

The Difficult Child and the Problem of Discipline. By C. W. VALENTINE. London: Methuen and Co., Ltd. 1940. Pp. viii+104. 4*s.* net.

This book is addressed mainly to parents and teachers, with a view especially to removing the misconceptions about child psychology which they may have acquired from the writings of foolish enthusiasts and sensational journalists. If indeed there are those who still swallow these stupid exaggerations and model their treatment of children upon them, the advice, which is lucidly given, should be valuable. One wonders if there are many, however—except the credulous and unbalanced individuals who are beyond the reach of good advice. It is perhaps a pity that the author gives the impression, in Part I of the book on Psychology and the Difficult Child, of underrating the value of the knowledge and understanding of children which psychologists have contributed, and of their ability not only to alleviate but to cure out of hand many of the maladjustments of childhood. It is not so much a question of normality or abnormality, but of removing in childhood difficulties which if neglected may impair adult happiness and stability. In these days Child Guidance (and all that it implies) needs every encouragement that it can get; and not until the last chapter of the book does the author really give it its due. In particular, he lays far too little stress on the vital importance of 'play' in the child's life. It is true that a direct interpretation given to the child of the meaning of his play should be undertaken only with caution and never except by the experienced play therapist. But parents and teachers should be urged to give all possible facilities and encouragement to free, imaginative and constructive play for the younger child, and occupations giving opportunities for imaginative expression in the older child. These can harm no one, and may help very many. Even destructive play may be valuable, and can be confined to suitable activities such as wood-chopping.

But these criticisms should not blind the reader to the interest and value of the book, particularly of Part II on the Problem of Discipline, which should be most helpful. It indicates how to steer a safe course between undue repressiveness and laxity in the treatment of children, stressing the importance of helping the child to acquire the measure of self-discipline possible at any given age. Prof. Valentine hedges a little on the problem of corporal punishment. The work of Hopkins may show what the child thinks about corporal punishment. But, apart from anecdotal evidence, has any one demonstrated any lasting *good* effect produced by it?

However, it is to be hoped that many parents and teachers will be guided and reassured by the sound conclusions that Prof. Valentine has so clearly and attractively expressed.

The Constancy of the Intelligence Quotient in Subnormal Children. By GILBERT E. PHILLIPS. Publications of the Australian Council for Educational Research. Melbourne University Press (London: Humphrey Milford, Oxford University Press). 1940. Pp. 86. 5*s*. net.

This monograph gives a rather lengthy and confused account of a piece of work, interesting in itself, which could nevertheless have been adequately described in a Journal. The intelligence quotients of 365 children at a residential Special school were measured, by means of the Stanford Binet test, at intervals over a period of nine years (though only 100 children actually completed five testings). The average I.Q. for the whole group was found to decline over this period. This was due, not to a progressive decline of all cases, but to the fact that certain children, particularly those of low I.Q. and unstable emotionality, 'matured' early. That is to say, their mental age did not increase regularly up to the age of fifteen, but became constant, in two cases before the age of eleven. It is clear, therefore, that prediction of the future intelligence level cannot be made with any certainty with such children, especially if tested only when quite young. However, for nearly 60% of the children tested, the Binet test gave a reliable measure of intelligence with a constant I.Q. up to fifteen years.

It seems a pity that no further study was made of the type of child who reaches this early 'maturity' ('standstill' seems a better term for it). But the conclusions reached seem well established and reasonable.

Psychological Studies in Dementia Praecox. By ISABELLE KENDIG and WINIFRED V. RICHMOND. Ann Arbor, Michigan: Edwards Brothers, Inc. 1940. Pp. x+211.

This interesting monograph is based upon the results of testing, with the Stanford Binet test, 500 hospitalized dementia praecox cases, together with a number of normal and mentally deficient adults for comparison. In addition, forty-one of the former, whose case histories are given, were re-tested after an average period of ten years. The purpose of the study was to find if there were any evidence of an irreversible deterioration in intellectual ability. The authors found none. Mental age averaged below normal, but many of the patients had shown poor intellectual ability during their school days before the onset of the psychosis. Previous suggestions were confirmed that such impairment of intellectual ability as appeared was functional only, and was largely determined both in degree and pattern by pre-psychotic intellectual capacity. There was no differential mental age score between the vocabulary test and the other Binet tests; thus the theory of Babcock that the former indicates the undeteriorated pre-psychotic mental level, as against the deterioration shown on the other tests, is not confirmed. On the other hand, performance in tests requiring relatively great expenditure of attention and effort was adversely affected by the withdrawn and inhibited state characteristic of the psychosis. Although the authors confined their studies to the results of Binet testing, it seems fairly clear that no structural deterioration of any particular mental 'faculty', e.g. imagination, or abstract reasoning, exists in dementia praecox, though there may be impairment of mental energy, due to emotional blockage.

Germany Possessed. By H. G. BAYNES. With an Introduction by HERMANN RAUSCHNING. London: Jonathan Cape. 1941. Pp. 305. 16*s*. net.

It is a matter of great interest to see how the Jungian school reacts to the horrors and chaos of the present war, which seems to provide so clear and obvious an example of the irruption of the 'collective unconscious' at its most primitive and violent. Dr Baynes has taken full advantage of this opportunity. In the first few chapters of his book it seems that here at last we are being given a coherent and consistent account of the application of Jungian theory to a large-scale practical case. The picture of Hitler as the 'shaman' who relies on his collective unconscious to release the inhibited functions of the collective unconscious of the German people, believing himself a Siegfried to the German Wotan, enables us to understand how so undistinguished an individual should be capable of such immense 'drive' and cleverness. But convincing as is the analysis of Hitler's mentality, by comparison the analysis of the mentality of his followers is relatively 'thin' and obscure. Perhaps it is impossible for us fully to understand why Hitler with his power lust appears to his followers as saviour rather than as madman; or to see how a whole, or the major part of, a nation can succumb to the intoxicated violence of unrestricted collective unconscious activity. Apparently the restraints of tradition and custom are adequate to prevent any similar breakdown in this country, without being so repressive as to inhibit all freedom of action. But in the latter part of the book the author seems somewhat to lose his grip of the argument, which becomes repetitive and moves circularly about the point from which it started rather than proceeding logically forwards. The definitive and clearly comprehensible exposition of the Jungian theory is yet to come. Nevertheless, both the psychologist and the general reader should find the ideas presented in the earlier part of the book interesting, suggestive and well worth studying. And it is of particular importance in providing a reasoned antidote to the superficialities of the scientific materialists, which are so inadequate to the understanding of the present situation.

The Psychodynamics of Abnormal Behaviour. By J. F. BROWN. London: McGraw-Hill Publishing Co., Ltd. 1940. Pp. xvi+484.

It would be most interesting to see some attempt at a systematic integration of the Freudian theory and Lewin's Dynamic psychology, since the two systems have much in common, and much to learn from each other. Both Lewin himself and his disciple J. F. Brown have written papers on the subject, but these have appeared in obscure

American publications unobtainable in this country. Consequently English readers will be disappointed after noting the title of this book to find that it contains no more than a brief and sketchy account in the last chapter of the similarities between the theories and findings of Freud and Lewin. The rest of the book consists of a rather uninteresting exposition of abnormal psychology, with sections on Symptomatology, The Theory of the Structure and Genesis of Personality (mainly the Freudian theory), and Psychiatry. It is no better and no worse than the many similar books on abnormal psychology; except that the author cannot evade his 'King Charles's head'—his dislike of Christianity.

Superstition and Society. By R. Money-Kyrle. Psycho-analytical Epitomes No. 3. London: the Hogarth Press and the Institute of Psycho-analysis. 1939. Pp. x+163. 4*s.* 6*d.* net.

This book gives a readable exposition of the Freudian interpretations of mythology, superstition, magic and taboo. It is mainly based on Freud's *Totem and Taboo*, but the original Freudian theory is expanded by Dr Money-Kyrle in accordance with his own views—those on anthropology being interesting and plausible, those on education less so. Whether this kind of popularization of Freudian explanations is desirable seems rather doubtful. The whole theory is too speculative and subject to modification —in parts symbolically rather than factually correct. Popular expositions give it a fictitious clarity, definiteness and conclusiveness which is often misleading.

Civilization, War and Death. Selections from three works by Sigmund Freud. Edited by John Rickman. Psycho-analytical Epitomes No. 4. London: the Hogarth Press and the Institute of Psycho-analysis. 1939. Pp. viii+102. 3*s.* 6*d.* net.

This book contains extracts from *Thought for the Times on War and Death* (1915), translated by E. Colburn Mayne; *Civilization and its Discontents* (1929), translated by Joan Riviere; and *Why War?*, an open letter to Prof. Einstein (1933), translated by Stuart Gilbert.

My Life. By Havelock Ellis. London: William Heinemann, Ltd. 1940. Pp. xviii+542. 15*s.* net.

Reading this book prompts the reflexion that the art of writing autobiography lies in selection. To describe certain parts of one's life and thoughts in the minutest detail (it is obviously impossible to describe all) is confusing to the reader, who is unable to see the wood for the trees. Moreover, the recounting of certain intimacies does not create an impression of frank disclosure so much as of a rather embarrassing exhibitionism. The conclusion is that Havelock Ellis can be best known and most highly valued from the sympathy and understanding displayed in his works on the psychology of sex.

The Myth of the Mind. By Frank Kenyon. Thinkers' Library No. 85. London: Watts and Co. 1941. Pp. ix+115. 1*s.* 3*d.* net.

This book is an exposition of the familiar hypothesis that the mind can be identified with the brain. To prove this, the author attempts to describe certain mental phenomena about which a fair amount is known in terms of central nervous processes about which very little is known; while ignoring those mental phenomena which cannot by any stretch of our present knowledge be treated in this way. It seems rather a pity that this book should be included in a 'Thinkers' Library'.

BRITISH PSYCHOLOGICAL SOCIETY

REVENUE ACCOUNT AND BALANCE SHEET AS AT 30 SEPTEMBER 1941

Revenue Account for the year ended 30 September 1941

1940 £ s. d.	1940 £ s. d.	EXPENDITURE	£ s. d.	£ s. d.
130 0 0		*To* Rent		122 10 0
17 6 8		Use of Room for Office		12 0 0
2 9 8		Lighting, Heating and Cleaning ...		11 10 8
252 0 0		Secretary's Salary		252 0 0
73 1 3		Printing and Duplicating		53 16 9
16 5 2		Expenses of Meetings		12 4 8
54 10 8		Postages and Telephone		78 8 5
21 5 3		Stationery		12 12 4
4 12 0		Insurance		4 10 10
3 8 3		Subscriptions to outside Associations		3 8 3
10 10 0		Audit Fee 1940		10 10 0
2 6 1		Teas		1 1 2
2 15 7		Sundry Expenses and Air Raid Precautions		8 12 1
		Journals Publication Expenses:		
	225 13 6	General	217 17 5	
		Medical	110 12 1	
	94 14 6	Educational	98 5 0	
320 8 0				426 14 6
15 3 8		Extended General Meeting 1941 ...		14 19 1
1 9 9		Midland Branch Expenses ...		5 0
7 19 7		Scottish Branch Expenses		9 10 4
1 0 0		Cheque Books		1 0 0
		Removal Expenses		35 4 9
42 18 1		Removal of Library to Nottingham		
13 5 0		Legal Expenses *re* Extension and Termination of Russell Square Lease		36 5 0
75 0 0		Legal Expenses *re* Incorporation		
		Depreciation:		
		Library	58 2 7	
85 13 1		Office Furniture and Fittings ...	22 1 5	80 4 0
1153 7 9				1187 7 10
269 1 7		Balance, being Surplus of Income over Expenditure		236 4 3
£1422 9 4				£1423 12 1

1940 £ s. d.	INCOME	£ s. d.	£ s. d.
	By Subscriptions 1941	1447 6 6	
	Subscriptions 1940	7 7 0	
		1454 13 6	
	Less Resignations and Removals ...	158 7 10	
1284 2 0			1296 5 8
125 4 10	Interest on Investments		127 6 5
13 2 6	Recovery of Income Tax		
£1422 9 4			£1423 12 1

Balance Sheet as at 30 September 1941.

1940 £ s. d.	LIABILITIES	£ s. d.	£ s. d.
	Sundry Creditors		27 15 5
12 10 0	Rent receivable in Advance		
	Subscriptions in Advance		9 18 6
	Publications Reserve:		
122 0 0	Balance as per last Balance Sheet ...		122 0 0
	Revenue Account:		
	Balance as per last Balance Sheet ...	5352 0 6	
5352 0 6	*Add* Surplus of year	236 4 3	5588 4 9
£5486 10 6			£5747 18 8

1940 £ s. d.	ASSETS	£ s. d.	£ s. d.
498 17 10	Cash at Barclays Bank Ltd.		616 13 5
1 12 2	Postage Stamps in hand		4 2 2
	Investments at Cost.		
	£300 5% Conversion Loan 1944–64 ...	318 9 9	
3743 17 6	£3514. 9*s.* 6*d.* 3½% Inscribed War Loan	3625 7 9	3943 17 6
	Note Market value of above Securities, at 30 September 1941, £4040		
45 0 0	Rent, 55 Russell Square, paid in Advance		
1 3 7	Sundry Debtors		2 16 1
244 4 6	Subscriptions in Arrear		293 18 6
	Office Furniture and Fittings:		
	Balance as per last Balance Sheet ...	209 18 6	
	Add Expenditure during year ...	11 12 11	
		221 11 5	
209 18 6	*Less* Depreciation	22 1 5	199 10 0
	Library:		
	Balance as per last Balance Sheet ...	561 3 0	
	Add Expenditure during year ...	22 9 7	
		583 12 7	
561 3 0	*Less* Depreciation	58 2 7	525 10 0
	Monograph Publications:		
	Balance as per last Balance Sheet ...	130 13 5	
130 13 5	*Less* Amounts repaid during year ...	18 17 5	111 16 0
50 0 0	Shares in the British Journal of Educational Psychology Ltd., at Cost ...		50 0 0
£5486 10 6			£5747 18 8

I beg to report that I have examined the foregoing Accounts with the Books and vouchers of the British Psychological Society and that I have obtained all the information and explanations I have required. In my opinion the Balance Sheet is properly drawn up so as to exhibit a true and correct view of the state of the Society's affairs according to the best of my information and the explanations given to me and as shown by the Books of the Society. The Securities appearing under the heading Investments have been verified by production of the Bankers' Certificate.

T. BARRETT,
Incorporated Accountant

29 October 1941.

S. J. F. PHILPOTT,
Hon. Treasurer

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VOLUME XXXII APRIL, 1942 PART 4

HEAD'S CONCEPT OF THE SCHEMA AND ITS APPLICATION IN CONTEMPORARY BRITISH PSYCHOLOGY

BY R. C. OLDFIELD AND O. L. ZANGWILL[1]

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I. INTRODUCTION

> Within my corner I will take my place.
> And grant me grace
> Some delicate thing to perfect and complete
> With passionate contentment as of old
> Before my heart grows cold.
>
> HENRY HEAD

IN the present state of psychological science the value of theoretical formulation and interpretation must be evaluated rather in terms of its fruitfulness in promoting experiment than by reference to its internal consistency. The time has yet to come, we believe, when large-scale conceptual systems can be erected with any likelihood that they will be found to correspond in detail with equally large bodies of fact. Accordingly, in endeavouring to trace the development and applications of the concept of *schema* we shall make criticism subordinate to exposition. Our object will be to provide a connected account of the sequence of ideas which, in the hands of Bartlett especially, has led to the provisional establishment of what we believe to constitute a truly novel approach to some of the fundamental problems of psychology. Where comment or criticism will serve to make clearer the ideas we desire to expound, we shall try to provide them. But where criticism is not forthcoming, it must not be assumed that we regard the views in question as being in any sense beyond its reach. Purely destructive analysis would, indeed, be as easy as it would be unprofitable. Constructive criticism, and the extension of the theory beyond its present published limits, we leave to the future.

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Historically, the theory must be regarded as in some sense a justifiable retreat of explanatory pretensions before a fuller apprehension of the complexity of presented fact. It arises essentially in the failure of nineteenth-century systems of interpretation. These, endeavouring to construct the basic mechanisms of perception, memory, thought and language out of elements which were the product of abstraction rather than experience, were eventually brought to a standstill by the failure of the picture they painted to resemble its original. This breakdown brought about a more thorough examination of the facts demanding-explanation, and greater caution in the use of conceptual schemes of interpretation. In the study of aphasia, memory and thought, to name but three instances, there arose a tendency to introduce new concepts, not for the purpose of erecting fresh scaffolds of deductive theory, but in order to permit the accurate characterization of phenomena within the limits imposed by available methods of investigation. The recognition that the description of fact in a manner suited to further progress is dependent upon the existence of an adequate conceptual basis is implicit in the work both of Head and of Bartlett. Implicit, too, is the hope that out of this somewhat fluid conceptual system there will eventually crystallize the means of attempting once again the construction of large bodies of explanation internally integrated by relations of a deductive character. But this step must wait upon the further development both of factual knowledge and of conceptual and linguistic invention. For the present the views we shall expound must be judged as a strategical retreat rather than a brilliant sally.

II. THE APPRECIATION OF POSTURE AND PASSIVE MOVEMENT

(1) *Munk's theory*

Head's notion of the *schema*[1] was introduced in the first instance in order to account for the appreciation of passive movement and the recognition of postural change. He maintained that the impairment of these functions found in certain cases of cortical injury could not be adequately interpreted in terms of the existing doctrines of cortical function, and singled out for detailed criticism the theory of Munk as representative of contemporary explanation.

On the basis of his extensive extirpation experiments, Munk (6) had advanced the view that the so-called *Fühlsphäre*[2] is directly concerned with the appreciation of touch, pressure, temperature and muscle sensations. With the exception of those of the last group, these sensations could be reactivated centrally in the form of images. The muscle sensations, though not themselves capable of imaginal revival, could nevertheless combine with the contact or pressure sensations to give rise to contact or pressure images. "And, moreover, the two kinds of sensations combined go to provide the definite and distinct images of the actual and momentary positions of the different parts of the body, and of the alterations in position of the various parts of the body in the case of passive movement" ((6), p. 32).[3]

On this view, the mechanism of active movement involves two sets of factors. The first comprises what Munk called sensations of innervation, and the second, images of movement. Sensations of innervation were defined as the perceptions of active movements of the various parts of the body. Munk's theory of their origin is somewhat curious. He supposed activity on the part of those spinal and midbrain centres concerned with the initiation of movement to be represented in consciousness as sensations of innervation. These, however, he believed always to arise in conjunction with the pressure and muscle sensations, and to become integrated with them in the elaboration of images of movement. "For each co-ordinated and adapted movement", he writes, "the three types of sensation stand in a specific and definite relationship to one another, and combined, they permit firstly the arousal of images of movement—the images of the active movements of the parts of the body—and secondly the arousal of the touch images—the images of the form, extension, etc.—of the objects which are touched by the moving members of the body" ((6), p. 33).

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[1] The choice of the term *schema* is, as Bartlett has pointed out, in many respects an unfortunate one in virtue of its many and varied connotations. In particular it is essential to point out that the term has nothing in common with Kant's use of the same word. On the other hand, it is perhaps worth noting that, in their functional though not in their generic aspects, there are certain resemblances between the *schemata* of Head, and the *pure intuitions* of Space and Time, as expounded by Kant in the *Aesthetic*. It is one of the functions of the Head *schema* to measure up and articulate the crude impressions of sense before they can be apprehended consciously. The Kantian pure intuitions have much the same function in articulating sense-impressions and rendering perception possible. Another example of the use of the term *schema* is to be found in the writings of Selz (13). There are certain obvious affinities between his use and that of Bartlett, but these will not be further discussed in this paper.

[2] Munk used the expression *Fühlsphäre* to denote the part of the cortex concerned with the initiation and elaboration of motor activity generally. In view of Munk's somewhat peculiar treatment of cortical function, it has been thought better to preserve his original term.

[3] We alone are responsible for the translations from the German of Munk.

The sensations of innervation were considered by Munk to be of paramount importance in the elaboration of images of movement. These images he supposed to be developed in infancy out of purely reflex movements; and, in later life, the images corresponding to movements of a given part of the body could arise in the *Fühlsphäre* despite the fact that this organ is not normally concerned directly with the initiation of the movements in question. Moreover, after extirpation of a specific region of the *Fühlsphäre*, Munk believed that the lost images of movement could be built up anew out of the reflex and locomotory movements of the body.

Munk claimed that the results of his extirpation experiments not only established his general theory, but also led to the conclusion that there is strict localization of these various *perceptual* functions in the *Fühlsphäre*. "The parietal lobe cortex of the dog is the *Fühlsphäre* for the opposite side of the body. It can be divided into a number of areas, with each of which is correlated some specific part of the corresponding side of the body. The fibres conveying the skin, muscle and innervation sensations from the side of the body in question terminate in the central, perceiving, elements of each such area, and within these areas, the images of the corresponding parts of the body have their seat. Each specific area thus constitutes the independent motor cortex (*Fühlsphäre*) of the corresponding part of the body" ((6), p. 38).

The effect of lesions in various parts of the motor cortex was explained by Munk in the following manner: "Small lesions in the various cortical areas result in partial loss of images of the corresponding part of the body; larger lesions the complete loss of the images in question. Psychic disablement (*Seelenlähmung*)—psychic paralysis and psychic anaesthesia—of the corresponding part of the body is the outcome. The lost images can nevertheless build themselves up anew in the remainder of the *Fühlsphäre*. The sensations are themselves affected in the event of more extensive extirpations, and in such cases the images can be reinstated only in part. The disturbance and incompleteness of restitution is greater the smaller the amount of cortex remaining intact. Total destruction of the *Fühlsphäre* corresponding to a given part of the body must inevitably result in the permanent abolition of all sensation and images appertaining to that part of the body. This is 'cortical disablement'—(*Rindenlähmung*)—cortical paralysis and cortical anaesthesia" ((6), pp. 38–9).

(2) *Head's criticism of Munk*

Munk's conception of the motor cortex as a repository of images of movement was sharply contested by Head (1), who maintains that the term 'image' strictly refers only to that which can be voluntarily recalled into consciousness. He points out that if we sit immobile, and imagine our fingers touching some object, the only image in consciousness is a visual one. Head then proceeds to adduce clinical evidence from cases of cortical lesion to the effect that appreciation of posture and of passive movement may be impaired or abolished without any loss of power to visualize the position of the affected part of the body. "Place the patient's affected arm in front of him on the bed, allowing him to see the position in which it lies; close his eyes, and in most cases he will see a mental picture of his hand. Then change its position while his eyes remain closed and he will continue to see a picture of the hand in its old position. Moreover, if localization is not affected, he will name correctly the spot stimulated but will refer it to the position in which he visualizes the hand. The visual image of the limb remains intact, although the power of appreciating changes in position is abolished" ((1), p. 605).

Head, whilst acknowledging that Munk was almost alone in perceiving the fact that some standard deriving from past experience is necessary to the appreciation of posture and of passive movement, argues that this standard cannot be a visual one. He likewise maintains that "images of movement", by which he appears to mean kinaesthetic imagery, cannot fulfil this function of supplying postural standards. It must, however, be pointed out that no experimental data are quoted in support of this last contention.

(3) *Head's concept of the schema*

In view of the considerations summarized above, Head attempts an alternative hypothesis: "...the image", he writes, "whether it be visual or motor, is not the fundamental standard against which all postural changes are measured. Every recognizable change enters into consciousness already charged with its relation to something that has gone before, just as on a taximeter the distance is presented to us already transformed into shillings and pence. So the final product of the test for the appreciation of posture or passive movement rises into consciousness as a measured postural change.

"For this combined standard, against which all subsequent changes of posture are measured before they enter consciousness, we propose the word 'schema'. By means of perpetual alterations in position we are always building up a postural model of ourselves which constantly changes. Every new posture or movement is recorded on this plastic schema, and the activity of the cortex brings every fresh group of sensations evoked by altered posture into relation with it. Immediate postural recognition follows as soon as the relation is complete" ((1), pp. 605–6).

(4) *Examination of Head's evidence*

Can we assess the extent to which Head's 'refutation' of Munk's doctrine is justified? In the first place we may reasonably ask whether the results of Head's observations upon the relation of imagery to postural recognition inevitably necessitate the abandonment of Munk's position. Head's experimental data, it must be emphasized, are relevant only to the question whether visual imagery plays any significant role in the appreciation of postural change. It will be remembered that Munk makes no explicit mention of visual imagery in this connexion, yet when he speaks of images of movement it is clear that he has some purely psychical product in mind. The upshot of Head's criticism is really that postural recognition in normal subjects habitually and demonstrably takes place without the intervention of introspectively detectable images of any kind whatsoever.[1] One may perhaps conclude that although

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[1] Prof. Bartlett, in discussing this paper with us, has called attention to a rather different interpretation of Head's position. He suggests that the presence or absence of images in consciousness is a relatively unimportant factor in the argument. What *is* important, however, is the presumption that images *cannot* furnish a standard of the kind required for appreciating position. An image, Bartlett argues, is always static and always constitutes a definite event in the continuity of mental life. Whatever items may be supposed to underlie postural recognition must, on the other hand, *literally disappear and reappear only in the result*—namely in direct awareness of position. The image, by its very nature, cannot furnish the necessary basis for assessing postural change.

Head has failed to produce an unequivocal refutation of the earlier theory, he has emphasized forcibly the highly improbable character of Munk's hypothesis.

III. HEAD'S THEORY OF THE SCHEMA

(1) *Posture*

The mechanisms of postural co-ordination must evidently involve activity at a number of levels in the nervous system. Among the many problems associated with these mechanisms, Head was pre-eminently concerned with the nature of those afferent co-ordinations which must directly underlie voluntary adjustive activity. In the first place, he points out, postural recognition is not constantly in the central field of attention, but always forms the measure against which we judge subsequent postural change. In the second place, he denies the possibility of a direct perception of position, and maintains that the appreciation of posture and passive movement must necessarily presuppose some background of comparison and relation. In postural recognition, awareness of an altered position is immediate, yet the postural sensations rise into consciousness "charged with the relation to something that has happened before". According to Head, immediacy of presentation on the one hand, and relation to the past on the other, can be adequately reconciled only through postulation of the sensory *schema*. We must therefore examine more closely the character of the *schema*, and the part Head believes it to play in the maintenance and regulation of posture.

The schema may perhaps best be envisaged as a 'plastic model' of the individual's own body in space and time—a kind of *homunculus* upon which movements are successively registered as they occur. A continually self-compensating basis of comparison is thus provided.

An analogy may perhaps best serve to make the essential nature of this view clearer. Let us suppose that we are driving a motor car upon a desert without any effective landmarks, and are equipped only with a milometer and a compass. We require to keep track of our position, but it is not practicable to drive along a straight course from our starting point. In the first place it is evident that, in the midst of the desert, 'position' has no effective significance except in relation to some fixed point through which we have previously travelled. And this is analogous to Munk's assertion that the recognition of posture cannot take place independently of the adoption of some standard deriving from the past. But the question analogous to that raised by Head is that of how it is possible for us at any given moment to determine our position. It is plain that merely reading the compass to determine the direction of travel, and the milometer to determine the distance travelled from a fixed point already passed through will not suffice. It is essential that both distance travelled and direction shall have been *continuously recorded* throughout the entire journey from the fixed point. This corresponds to Head's assertion that no single element from the past will suffice as the basis of recognition of posture. The standard must be a continuously variable one, involving in an important sense all that has gone before. But, in addition, if this continuous record has in fact been kept (and it is easy to imagine some mechanism by which compass and milometer are made jointly to move a point recording on paper), then there is no need for any calculation in order to determine our position, since it is directly indicated upon the recording apparatus. This is analogous to Head's statement that there is direct perception of posture unmediated by any preliminary conscious activity. It is of the very nature of the schematic mechanism, which allows any recognition of posture to be made at all, that it affords that recognition directly. It is in some such way as this that we must envisage the concept of schema.

Head makes it plain that schemata are to be envisaged as *physiological* dispositions.[1] "...the activities on which depend the existence and normal character of the schemata lie for ever outside consciousness; they are physiological processes with no direct psychical equivalent" ((1), p. 723).

The part played by the schema in the maintenance and regulation of posture may perhaps be summarized in something like the following terms: In the first place, incoming impulses from the periphery are continuously recorded on, or by, the schema; a tally is thus kept of the state of the organism at every moment which affords the necessary basis for the proper use of those impulses that follow. In the second place, the activity of the cortex brings every fresh group of sensory impulses evoked by altered posture into relation with the continuously altering record provided by the schema. Postural changes may, or may not, invoke changes in the central field of consciousness. In either case the initiation of change demands the utilization, on the physiological level, of the product of reaction between incoming impulses and schema. If the change does make itself felt in the central field of consciousness, then such conscious change is the direct product of an already completed reaction between present and past carried out by the activity of the cortex at the physiological level. The process whereby incoming impulses and schemata react, whilst purely physiological, involves both comparison and relation, and may be regarded as *analogous to* a conscious act of comparing and relating. Finally, in the third place, the incoming impulses themselves modify the schema, leaving their own record in a form already related to the past, and so putting the mechanism in a state of readiness to deal with the next incoming group.

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[1] Head refers to postural schemata sometimes as *dependent upon* physiological dispositions, sometimes as the dispositions themselves. In saying that the schema is *dependent upon* the physiological disposition, we believe that Head was emphasizing a purely formal aspect of the operation of these dispositions, and did not intend to postulate a distinct though dependent entity. Thus one may distinguish the shape of an organism from the material constituting it despite the obvious dependence of the former upon the latter.

The existence and proper operation of the schemata depend upon the functional integrity of the cortex. Cortical lesions, by impairing or destroying the schemata, concomitantly abolish the recognition of posture on the affected side of the body. "Not uncommonly a patient with a cortical lesion can recognize that some movement has occurred, but is entirely unable to discover its direction or amplitude; complete appreciation of passive movement is the recognition of serial changes in a certain direction" ((1), p. 604).

One example of defective appreciation of posture has already been given (vide p. 271 supra). In another case quoted by Head, a leg was lost some time before the appearance of the lesion which destroyed the power of recognizing posture. Sensations of movement in the phantom leg ceased on the occurrence of the cerebral lesion—"the stroke which abolished all recognition of posture destroyed at the same time the phantom limb" ((1), p. 606).

While the schemata are thus subject to impairment or abolition in consequence of certain injuries to the cortex, their existence also demands a *continuous* supply of incoming impulses from the part of the body concerned. A patient whose little finger was completely deprived of sensibility, owing to section of the ulnar nerve by gunshot, recognized the existence of this finger as a dead part of himself. After five months, however, it became necessary to amputate through the lower third of the arm. The phantom hand which appeared subsequently had only four digits. "During the five months of total insensibility, the schemata associated with the little finger, no longer reinforced from the periphery, had gradually died away..." ((2), pp. 489–90).

Beyond thus indicating the general physiological conditions upon which the existence and functioning of the schemata depend, and introducing in this connexion the concept of 'vigilance' (of which a brief account will be given later), Head does not attempt any closer enquiry into their anatomical and physiological nature. On the other hand, the destruction of schemata does not exhaust the functional disturbances entailed by cortical injury. Manifesting itself in combination with the consequences of schematic destruction, but essentially independent of them, is the loss or impairment of what Head calls *local attention*. If psycho-physical judgements regarding sensations from the affected part of the body are required, the patient's responses are uncertain and variable. Hallucinatory sensations are apt to appear in the absence of an appropriate stimulus. Sensations initiated by a stimulus may persist beyond its duration, and the intensity of a sensation is not graded according to the strength of the stimulus. In fact the patient's behaviour, Head considers, presents an exaggerated picture of the normal individual giving psycho-physical judgements while in an inattentive state of mind. On this account, he suggests that the defect consists in a loss of 'local attention'. "When the affected part is under examination, the patient's replies are uncertain and variable,...and yet, as soon as we examine any unaffected part, the patient may come through the same series of tests without a mistake[1]...but the patient's power of general attention has not been lowered....Records from the limbs of the sound side show that the phenomena of defective attention are confined to the abnormal part. A portion of the patient only has, as it were, become untrustworthy" ((1), pp. 606–7).

In summing up, Head concludes that "...the cerebral cortex is the organ by which we are able to focus attention upon the changes evoked by sensory impulses....Uncertainty of response destroys all power of comparing one set of impressions with another and so prevents discrimination" ((1), p. 607). It would appear, as we have already noted, that Head regards this consequence of cortical lesions as distinct from those connected with the destruction of schemata. In particular, according to his analysis, it concerns the level of consciousness. But we believe that it would be possible reasonably to interpret what appears behaviourally as loss of local attention in terms of the failure of the process of reaction between schemata and incoming impulses. Thus the sensations concerned in the psychophysical judgement would arise in consciousness lacking in that essential determination in respect to what has gone before, which alone can make them the effective conditions of an accurate judgement. It is possible, indeed, that some such view was in Head's mind, but, if this be the case, his use of the notion of local attention, even as a figure of speech, is definitely misleading. In any case, it will be unnecessary to consider further this type of defect.

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[1] That is, without making mistakes in relation to the relative intensities of supraliminal stimuli.

(2) *The maintenance of static tone*

The presentation to consciousness of data relating to the position of the body in space, and the indirect regulation of its movement, do not constitute the only functions of the postural schemata. They are also held responsible for the maintenance and regulation of tonic innervation. "The only constant and continuous record of our bodies in space", writes Head, "exists in the condition of our schemata. These physiological dispositions of the cortex ensure that the inception of a voluntary movement will find the part to be moved in a suitable attitude. For, unless postural impulses perpetually modified these unconscious physiological activities consonantly with every change in position, we might will a certain movement although the limb was not in a suitable attitude to bring it into being....But in order that the part of the body to be moved may be ready to spring off, like a runner, at the word of command, its static tone must be normal. Should it be poised atonically, the voluntary motor act has first to gather in slack before the limb begins to move. Normal posture and normal tone are coincident terms" ((1), p. 724).

Head considers the maintenance and co-ordination of static tone to be the product of a "constant stream of influence" exerted by "the simple existence of the physiological dispositions which we have called schemata". He supposes this automatic influence to be exerted directly upon the highest efferent centres without the intermediation of consciousness, and shows that it may be grossly disturbed as a consequence of cortical lesions. "It is obvious", Head observes, "that any lesion which disturbs postural schemata will interfere with static tone. For in order that a part of the body at rest may retain a normal posture, afferent postural impulses must exert a constant influence on the activity of the appropriate receptive centres of the cortex. These are the repository of spacial[1] schemata. The physiological changes brought about by this stream of afferent impressions not only checks and controls (*sic*) voluntary movements, but ensures that the static tone of the part shall be adapted to maintain its position. Consciousness is in no way necessary for such co-ordination; in fact the regulation of tonic innervation occurs entirely on the physiological level. Any lesion, which tends to destroy postural schemata, not only disorders voluntary movement, but under suitable conditions may diminish static tone" ((1), p. 724).

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[1] Head frequently uses the expressions "spacial schemata" and "postural schemata" interchangeably. It would appear to us that by the expression "spacial schemata" Head meant to include not only the postural schemata already described, but also the schemata concerned with projectional surface localization. These will be briefly dealt with in the next section.

The hypotonia so closely associated with defective recognition of posture and passive movement in the absence of any defect in the highest motor centres, Head treats as a false co-ordination. In the first place, he points out, the receptive centres in the cortex are "badly informed" of the position of the affected parts. (By this, he presumably implies some direct interference with afferent pathways.) In the second place, the schemata which exist in these receptive centres are themselves disordered. Such disturbance results in defective co-ordination of voluntary movement and concomitantly affects the static tone of the parts concerned. The effect is contrasted by Head with the disturbance which may result from a subcortical lesion, where the permanent dispositions of the afferent centres remain intact, and gross impairment of static tone may be absent, unless it be the result of pathway interference ((1), pp. 724–6).

(3) *Projection*

By projection Head means, broadly, the reference of a sensation to an object. His theory of the projectional aspects of sensation and of the schemata which underlie them is intimately bound up with his general theory of sensation. The latter involved the distinction of protopathic and epicritic types, and made much use of the Jacksonian concept of levels.[1] Further, Head insisted throughout upon the complexity of the integrative processes and the competition of incoming impulses at the highest physiological levels. All these diverse aspects of the general theory must be borne in mind in reading the discussion that follows.

As we have said; broadly speaking projection is the reference of a sensation to an object. But as reference is complex, an injury or disease may abolish some aspects of projection, leaving others intact. If a normal subject be pricked with a pin, he will recognize that this is so; the sensations are referred to the pin. In addition, the position of the pin will be more or less accurately localized. The force with which the pin is pressed against the skin will affect in a more or less simple and regular manner the intensity of the sensation. But these "spacial attributes, relative intensity and individual character" of the sensation are dependent upon the integrity of the cerebral cortex, whose function it is to mediate them. "When the influence of the cortex is removed", writes Head, "and the optic thalamus exerts its activity uncontrolled, the patient may cease to associate his sensory experiences with any external agency. He complains that he is being hurt, or that something is happening to him, but fails to recognize that he is being pricked with a pin. Removal of the cortical factors has reduced to elementary proportions the power of projection, as we know it in the intact human being. It is no longer possible to recognize the size, shape, weight, and spacial relations of an external object,[1] nor, indeed, to appreciate the relative intensity of the stimulating action it excites" ((1), pp. 750–1).

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[1] Head's intellectual debt to Jackson is so many-sided that it becomes difficult to assess in the treatment of any one problem. But it is certain that the concept of schemata was foreshadowed in Jackson's general theory of third-degree cortical representation and it probably owes much to his notion of 'constants' (cf. Jackson (3), especially pp. 41–2, 54, 96, 98–118). While we have not attempted to trace the evolution of Head's thinking from Jackson's views, it is plain that the concept of schema is not only consistent with, but actually presupposes, the Jacksonian theory of cortical representation. We may also note that Pick (9), in an early exposition of the concept of the schema, expressly acknowledges his indebtedness to Jackson.

Head believes the simplest projected aspect of sensation to be shown in the power to localize the site of a stimulated spot on the surface of the body. As in the case of postural recognition, this ability must, he maintains, presuppose a process of schematic relation. Moreover, appreciation of position may be lost without a corresponding loss of ability to identify the locality of the stimulated spot on the skin. For example, "...a patient may be able to name correctly, and indicate on a diagram or on another person's hand, the exact position of the spot touched or pricked, and yet be ignorant of the position in space of the limb upon which it lies". Head therefore concludes that this faculty of localization must be associated with the existence of another schema or model of the surface of our bodies.[1] This schema may, however, also be destroyed by a cortical lesion, and under such conditions the patient complains that he has no idea where he had been touched. "He knows that a contact has occurred, but cannot tell where it has taken place on the surface of the affected part" ((1), p. 606).

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[1] Head points out that these attributes of sensation do not depend primarily on psychical functions. For he holds that, on the purely physiological level, "...afferent impulses possess projectional characteristics. The same class of impressions which underlie discriminative sensibility can be discovered at work controlling and regulating purely unconscious actions....Study of the reflexes at higher functional levels shows that they are governed by remarkably complex projectional relations....Thus...afferent impulses can be shown to be adapted to spacial conditions and to the intensity and relative character of the stimulus, although the whole procedure remains outside consciousness. If, however, they succeed in reaching the highest receptive centres, they endow sensation with spacial attributes, relative intensity, and individual character" ((1), p. 751). Head nowhere states explicitly that at all levels projection involves schematic mechanisms and it will be remembered that the locus of these is in the level of the highest receptive centres. Bartlett, however, seems to have assumed that schemata operate on *all* functional levels (vide Part II of this paper).

Associated with projection by reason of its similar basis in the existence of the spacial schemata is the complementary function of *introjection*. This is the power of extending the recognition of posture, movement, and locality beyond the limits of our own bodies to the extremity of an instrument held in the hand, or otherwise attached to the body. Without the spacial schemata, Head writes, "we could not probe with a stick, nor use a spoon unless our eyes were fixed on the plate. *Anything which participates in the conscious movement of our bodies is added to the model of ourselves and becomes part of these schemata*; a woman's power of localization may extend to the feather in her hat" ((1), p. 606; italics ours). In the same way, no one can drive a motor car effectively until intermediate processes by which his own bodily movements affect the behaviour of the car have been eliminated, and the vehicle has become a 'part' of its driver's 'body'.

Head's treatment of projection and localization is somewhat complicated by the introduction of his theory of protopathic and epicritic sensibility. On the basis of his classical experiment in nerve division, Head had advanced the theory that cutaneous sensation is dual in character, the two types being named by him epicritic and protopathic. While the epicritic is *par excellence* the system associated with projection, there is at the protopathic level an elementary form of this function. This is shown by the fact that when protopathic cutaneous sensibility alone is in operation "Any stimulus capable of evoking a cutaneous response aroused a widespread sensation, referred to some remote part of the affected area" ((1), p. 752). The sensation had the quality of extensiveness, although the amount of extension bore little relation to the actual size of the area stimulated. Thus, while the stimulus is wrongly localized, and its size incorrectly assessed, nevertheless it possesses in some degree those characteristics of determination which alone can make it relevant to speak of *error*. Moreover, "...this erroneous localization was not fortuitous, but remained the same whatever the stimulus employed..." ((1), p. 752).

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[1] This aspect of the schema theory has been elaborated at length in the writings of Pick(8, 9, 10). Pick appears to have developed his theory independently, but we are unable to ascertain whether he was in personal contact with Head before the appearance of Pick's first contribution in 1908. In his later writings, Pick adopted Head's conception of the schema in place of his own earlier notion of a 'spacial image' of the body. But he extended the theory to cover a much wider range of nervous and mental symptoms. In this, he has been followed by Schilder(11, 12). The notion of the 'body schema' (or 'body image') has, in fact, come to enjoy something of a vogue in contemporary neuropsychiatry. But the implications are so wide that they cannot profitably be discussed here. Adequate expositions of the general idea, together with much relevant clinical material, are to be found in the writings of Schilder(12), Tchehrazi(14), and Lhermitte(5).

Head points out that this limited form of projection found in the protopathic system may be regarded as providing a primitive form of defence mechanism. It is essentially segmental in character, the sensation, erroneously localized as it may be, always being referred to some place in the same segment as that to which the stimulus is applied, and the response being in general one of compulsory withdrawal of the part concerned. It is only with the development of the *epicritic* projectional systems that choice of response is introduced. Thus in view of the suprasegmental character of these systems, flight of the organism as a whole becomes possible.

We have seen that in the epicritic projectional system sensation is endowed with those characters which allow discrimination and thereby permit choice of response. In addition, however, it is the function of this system to control protopathic activity. Involved too in projection is the capacity of the organism to relate sensation to external objects. An "object", Head writes, may in fact be defined as "a complex of projected responses". It is said to have characters such as size, shape, weight, and spacial position which distinguish it from all other objects. "The recognition of such features, however, depends on physiological activities, the product of certain definite centres in the cortex. If these processes are unable to influence consciousness, the 'object' disappears, although its affective and qualitative aspects still produce their appropriate sensory reactions" ((1), p. 754).

We may infer that the afferent co-ordinations underlying epicritic projection are fundamentally schematic in character. For inasmuch as they confer upon sensation determination in respect of spacial attributes, intensity, etc., they involve comparison and relation to something that has gone before, or is co-present. "...all the higher projectional aspects of sensation...form a continuous series of dispositions determined by previous events of a like order. The unit of consciousness, as far as these factors in sensation are concerned, is not a moment of time, but a 'happening'. This consists in a group of occurrences belonging to profoundly different orders in the psycho-physical hierarchy"[1] ((1), p. 754).

Appreciation of temporal sequence represents yet another function attributed by Head to the existence of schemata. "The sensory activities of the cortex", he observes, "are not only responsible for projection in space, but also ensure recognition of sequence in time. For this would be impossible if a sensory impression had no necessary beginning or termination. One of the commonest defects produced by a cortical injury is this want of temporal definition; a stimulus, rhythmically repeated, 'seems to be there all the time'. The patient cannot appreciate the moment at which it is applied or removed. There is no complete recognition of an extended sequence of events" ((1), p. 754).

Head therefore concludes that it is to the projected elements in sensation that we owe our conceptions of coherence both in space and time. These factors are not due essentially to "judgement or conscious association", but depend to a great extent on physiological activities and dispositions. "When these are permitted to excite consciousness", he concludes, "they appear as an ordered sensation, related to other events in the external world and extended serially in time" ((1), p. 755).

It will now be evident that there is more in Head's notion of projection than the mere association of a stimulus with an external object, or other unified source of causal influences. The different contributions made by what he calls projection to perceptual consciousness may, broadly speaking, be listed as follows in order of complexity: (1) The simple localization of a stimulus—that is, the reference of the present sensation to a causal origin having position in regard to a frame of reference. (2) Appreciation of the extensity of the stimulus—that is, the recognition that the causal origin of the sensation is extended in a certain way in a field of spacial relations. (3) The identification, or superposition, of the causal origins of a number of different sensations—that is, the recognition that two or more impressions emanate from the same source of causal influence, or from two or more sources localized in the same region and essentially interconnected. (4) The recognition in general that the whole external field of influences is structured by spacial and other relations. (5) Similar recognitions as (3) and (4) in regard to temporal sequence and relations. (6) In virtue of all these, appreciation of the 'object-character' of stimuli.

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[1] The similarity to Kant is here especially apparent. In addition, however, to employing an essentially Kantian argument to the elucidation of the conditions of perception, Head is able to demonstrate the dependence of the perceptual characters in question upon the functional integrity of parts of the nervous system, and exhibit cases in which just these characters may be lacking.

The true order of complexity of these functions of projection is, of course, a matter for further investigation. And it may be that they should not be separated according to the scheme suggested. Head himself says that simple localization on the surface of the body is the simplest of the functions, but does not trace their relationships further. And it is possible, for instance, that appreciation of 'object-character' may precede and not follow appreciation of the general structure of the environment.

It is important to make clear the dual role of schemata in elaborating the projectional aspects of sensation. On the one hand they have to provide the general frame of reference, or dispositional system, appropriate to each aspect.[1] Thus the localization of a stimulus is meaningless except in regard to a frame of spacial relations and this itself demands an enduring physiological mechanism. Perception of, and behaviour towards, the 'object-character' of a stimulus is similarly impossible in the absence of dispositional systems providing the general basis of such perception and behaviour. Incoming impulses from the periphery contribute, to the activity of the schemata in the cortex, the particular clues mediating the perception of the particular place in which a stimulus is localized. Similarly they mediate the perception of the particular 'individual character' of an object in the environment. But they cannot determine that the stimulus shall be localized at all, nor that an object shall be perceived as such. This, then, is the first function of the schemata, to provide permanent, yet continuously modified, physiological dispositions which, acting in co-operation with the immediate clues, can endow perception with the determinativeness of which we are in fact aware.[2] The second function is the active one of mediating particular perceptions so determined.

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[1] We may note that C. S. Myers, as early as 1911, took over Head's concept of the schema in an interesting attempt to account for certain aspects of visual space perception, and, in particular, the auto-kinetic sensation (see (7), pp. 215–17, 231–2, 282–3, 294). Myers's approach has never been fully followed up, though Koffka has argued brilliantly on kindred lines in developing his theory of the 'spacial framework' (see Koffka (4), Chs. 5–7). These two approaches have much in common which would repay careful study.

[2] The schemata thus act *as if concepts* such as those of space and object were involved. This aspect of the functioning of schemata emerges again in Wolters's theory of 'conceptual thought and is discussed in Part IV of this paper.

(4) *Vigilance*

The term 'vigilance' was introduced by Head to denote a state of high-grade physiological efficiency of any given functional level of the nervous system. The state of efficiency of a given level is to be assessed by comparison with the maximum degree of adaptive function of which that level is capable. Thus in the *acute* decerebrate preparation there is a temporary lowering of vigilance, due to operational shock. This results in absence of the rigidity characteristic of the *chronic* state. When recovery has taken place, and the hind brain centres are again active in making their full contributions to the adaptive behaviour of the organism, namely the production of decerebrate rigidity, then the vigilance of this level is again said to be high. A high state of vigilance is associated not only with increased reaction but also with the optimal fulfilment of those functions delegated to the level of the nervous system under consideration. In the spinal cord these functions are those of reflex determination, and it is by the perfection, complexity and adaptive significance of the reflexes that can be elicited that its vigilance is to be assessed. In the hind brain the function concerned is that of affording the general basis of postural behaviour upon which may be imposed the niceties of adjustment afforded by the activities of the higher centres. Thus, in the decerebrate animal, the full development of rigidity is the expression of a high state of vigilance. The cortex is pre-eminently the organ of fine discrimination and consequently it is in the performance of the highest grade activities that its vigilance is displayed.

The vigilance of a given level of the nervous system may be reduced by a number of conditions. Fatigue is one; others are the toxic states produced by narcotic drugs and inflammatory conditions. Lastly there is structural damage. In this case, the lowering of vigilance is not restricted to mechanisms especially associated with the site of the lesion. The lowering of vigilance produced by cortical damage would seem to be comparable to the incidence of '*Diaschisis*' postulated by von Monakow,[1] at least in results, if not in mechanism. In any case change in vigilance is a physiological, and essentially a reversible, process (although, of course, the reverse process may occupy very considerable periods of time, and may never be completed).

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[1] Head appears to think that this comparison is not truly valid, although his comments upon von Monakow's views are not very specific ((2), pp. 84–93). Von Monakow's conception involves a withdrawal from some parts of the cortex of facilitatory influences originating in other, now damaged, parts.

In virtue of the hierarchy implied in the Jacksonian concept of levels, it is evident that, where general "conditions unfavourable to physiological activity" subsist, a lowering of vigilance will first be apparent through failure in the highest grade performances of the organism. "The more highly differentiated the act, the greater degree of vigilance does it require and the more easily can it be abolished by toxic influences... or by other conditions unfavourable to physiological activity" ((2), pp. 486–7). There is thus a sense, employed by Head, in which the notion of vigilance can be applied to the organism as a whole. But it is important to bear in mind that, primarily, it applies to functional levels in the nervous system.

At the cortical level, lowered vigilance, whether it be due to general or local conditions, may inactivate, and even abolish, the postural schemata. "...If the vigilance of the so-called 'sensory' cortex is lowered, either temporarily or in consequence of organic destruction, not only will postural sensibility be disturbed in the appropriate parts of the body, but their static tone will be diminished. This is not due to loss of postural sensation, but to the destruction of those physiological processes on which they are based" ((2), p. 489). Thus, in general, if we exclude those cases in which the structural damage itself must be held directly responsible for the impairment of function, the abolition of postural schemata is to be envisaged as an *indirect* outcome of cortical injury. The *immediate* consequence of the lesion is reduction in that condition of high-grade physiological vitality called by Head 'vigilance'.

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A SCIENTIFIC APPROACH TO MUSICAL AESTHETICS

By CARL E. SEASHORE

I. *Science can clarify and define essential concepts in aesthetics* (pp. 287–290).
II. *Science broadens the horizon for insight into the full nature of the aesthetic situation* (p. 290).
III. *Science creates a feeling of confidence in the tangibility of aesthetic issues* (pp. 290–291).
IV. *Science aids in dealing systematically with aesthetic problems* (pp. 291–292).
V. *The method of natural sciences is applicable in musical aesthetics* (p. 292).
VI. *The method of physical sciences is also applicable in musical aesthetics* (pp. 292–293).
VII. *Science in aesthetics proceeds by the operational method* (p. 293).
VIII. *Science encourages co-operation with all other legitimate approaches* (pp. 293–294).

In the current number of this *Journal* James Mainwaring has given a comprehensive criticism of the empirical approaches to aesthetics.[1] It is a clear presentation of a point of view often voiced and should be reviewed critically by the various interests concerned. Instead of attempting a formal criticism, let me set out a single constructive illustration of one empiricist's point of view in defence. In this I shall limit myself to a single concrete case dealing with the significance of a tonal spectrum in musical aesthetics from the laboratory point of view; and, for the sake of clarity and brevity, I shall limit myself to a few categorical statements which any competent investigator in the field of psychological acoustics can verify and evaluate in relation to the position taken by Mainwaring.

I. SCIENCE CAN CLARIFY AND DEFINE ESSENTIAL CONCEPTS IN AESTHETICS

The term 'tonal quality' represents one of the large categories of musical aesthetics. It has two components, namely, timbre and sonance. Timbre is a cross-section of a tone as represented by a single sound wave in terms of its harmonic structure, fundamental frequency and total intensity. Sonance represents the quality of a tone as determined by the harmonic structure of the successive waves in the tone as a whole. Thus timbre is a case of simultaneous fusion of partials at a given moment in the tone; whereas, sonance is a case of successive fusions of changing harmonic structures during the maintenance of a musical note. The present illustration will deal only with timbre, a term which, when understood in musical aesthetics, should be as useful as the terms pitch or rhythm. A parallel case could easily be made for sonance, thus covering the entire concept of tonal quality.

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[1] *Brit. J. Psychol.* (1941), XXXII, 114.

As a result of the recent extraordinary developments in acoustical techniques, the investigator can now select a fair sample of a note in its actual musical setting and take a highly detailed moving picture of every sound wave in that tone as it is generated. Realizing that everything that is conveyed from the performer to the listener is conveyed on sound waves, he has here the material basis for the analysis and clarification of all possible elements in the musical tone as an art object. The form of the sound wave is the physical basis of timbre. He can take as a fair sample any representative wave in the tone and run it through an harmonic analyser which will, in a single process, determine the number of partials present, their distribution, their relative formant grouping, and the amount of energy represented in each. These readings on a series of dials can be transferred into a table: The facts thus established can be represented in a single graph called a tonal spectrum.

He can convert this physical spectrum into a psychological or musical spectrum which, instead of showing the amount of energy in each partial, represents the audibility or loudness of each partial in terms of decibels. The art of making this transformation in accordance with the psychological and acoustic laws of hearing is a very recent contribution. Scientific aesthetics must employ both physical and psychological spectra; however, for the present purpose we shall think only in terms of the physical spectrum. From a spectrum the investigator can derive a complete, detailed, verifiable description and definition of the tone in cross-section, and in terms of the sequence of spectra he can give an account of the qualitative character of the tone as a whole. An experience of beauty should, of course, be couched in so far as possible in psychological terms. Likewise, the description of a physical tone should be couched in the established terminology of acoustics.

One of the greatest sources of confusion in aesthetics is abuse of language. In musical history we find scores of synonyms as substitutes for the term tonal quality, practically all loose and ill-defined. When we once understand the nature and significance of the tonal spectrum as the basis of timbre and sonance, we can scrap all these inconsistent terms and thus establish a common usage among scientists, artists and philosophers.

To the scientific worker, the tonal spectrum becomes a tool as familiar as the photograph of a friend's face; a single glance at the spectrum immediately gives him a clear concept of the quality of the tone represented in cross-section. It may be as exact and as easily understood as a commonplace mathematical equation. When acoustic science has developed such technical terminology, that terminology soon tends to become popular so that the teacher or the artist as well as the composer can readily understand what a spectrum means, can formulate descriptions and definitions in terms of it, can criticize or instruct in tonal quality in terms of that picture, and can think and speak with scientific precision about the beauty or ugliness of a specific tone. For this purpose it is not necessary that he should go through the process of deriving the tonal spectrum any more than it is necessary for the pleasure driver of an automobile to be an expert in automobile engineering. Such transition from the purely technical concepts to popular and practical spheres is one of the splendid examples of growth in the spread of learning.

Current attempts to name tonal qualities have resulted in such crude terms as rich and pure, smooth and rough, big and small, but these are inadequate. We have such devices as speaking of the different vowel qualities, bearing in mind that the differences involved are differences in formant regions. Then in desperation we go one step farther and speak in terms of instruments, such as the flute tone, the trombone tone, or a violin tone, ignoring the fact that each of these instruments is capable of producing hundreds of differences in quality. All these devices are more or less beggarly. But if we proceed on the assumption that all differences in tonal quality are representable in the spectrum, we shall have made great progress in laying foundations for a consistent and permanent language of musical aesthetics.

Students of aesthetics may therefore fairly ask if it is not possible to discover or set up a consistent scale of tonal qualities named in terms of the variables of the tonal spectrum. We think this can be done when the facts are sufficiently in hand. For example, we could recognize as many types of spectra as there are letters in the alphabet and name them alphabetically. Then a person talking about tonal quality could refer in stabilized language to the spectra as A, B, C type on the analogy of names for vitamins or possible combinations of these, such as the

A–K type. Finally, descriptive names might be assigned to each for a given gamut of definable differences.

II. SCIENCE BROADENS THE HORIZON FOR INSIGHT INTO THE FULL NATURE OF THE AESTHETIC SITUATION

One of the objections recently made to the experimental method is that the laboratory experimenter raises more questions than he can answer. That is one of the great merits of the scientific method. The experimental psychologist would go beyond the prevailing adage, that if you ask one question of nature nature will ask you ten, and say he can set up a sample experiment in aesthetics which will raise a hundred other relevant questions not previously thought of. When Wundt was asked what he had learnt from the reaction-time experiment, he replied that it had given him an entirely new concept of the human mind. So I would say that while the experimentalist in the laboratory can give a final and verifiable solution to only one very minute problem at a time, one of the great merits of the experimental procedure lies in the fact that it forces upon the horizon a vast array of issues which come as corollaries to the situation solved. It is obvious that recent progress in experimental psychology has performed this very function for aesthetics by generating a deeper insight into the nature of experience and expression of aesthetic emotions, and the nature of the relation between the aesthetic object and one who creates or feels beauty or ugliness in it. Would it be unjust to say that the laboratory empiricist is to the non-empiricist in aesthetics as the astronomer is to the stargazer? Both look into the starry heavens but they see entirely different worlds of beauty.

III. SCIENCE CREATES A FEELING OF CONFIDENCE IN THE TANGIBILITY OF AESTHETIC ISSUES

Many of the serious books on aesthetics now in our libraries take the same attitude toward aesthetic experiment that the forerunners of modern psychology took toward psychological experiment a hundred years ago: mind is so different from matter; beauty is such an ethereal and fleeting situation; a feeling of beauty is never constant; after all, beauty deals with subjective values, not objective facts; problems that can be solved in the laboratory are only an infinitesimally small aspect of the aesthetic situation; like the historical soul which was not subject to experiment, beauty is a sort of ultimate reality in itself, intangible.

But when the theory of evolution came upon the horizon, there was a sudden about-face in philosophical psychology and experimental psychology became an inceptive science. Fechner and his followers in experimental aesthetics committed many a blunder, but they made progress through trial and error which resulted in the gradual acceptance of the feasibility of scientific experiment in aesthetics.

The critic must distinguish between empirical and experimental. Great discredit is rightly thrown upon aesthetics by the publication of empirical studies of beauty which do not conform or measure up to the sanctions of scientific procedure. They bring discredit upon scientific method, as, for example, in the case of securing judgements about likes and dislikes of presumably beautiful objects without setting any experimental control to determine on what feature the aesthetic judgement is based, in judgements which ignore the influence of the total situation in which the experience occurs, in questionnaires which are certainly subversive of scientific procedure, and in the statistical treatment of uncountables. But such are the fumblings not only in an inceptive applied psychology but also in an inceptive art or philosophy.

However, none can dispute that a fundamental tool has objective validity as in the case of the tonal spectrum. The student of musical aesthetics must take notice, and what is more, develop a faith and confidence in objective and verifiable procedures. Twenty years ago I despaired of the possibility of seeing in my lifetime a relatively exact science of tonal quality. But modern acoustics has been revolutionary in its progress, and our present equipment in the laboratory now enables us to face that problem with enthusiasm and feelings of certainty in findings.

IV. SCIENCE AIDS IN DEALING SYSTEMATICALLY WITH AESTHETIC PROBLEMS

Take again the problem of the sources of beauty or ugliness in tonal quality as represented by the spectrum, and consider one of the elements, such as the formant, that is, the character of the grouping of dominant partials. Analysis of this problem shows that the principal variables in the formant are the number of formants present, the position of the formant, the width of the formant, and the relative prominence of the formant. These are indisputable facts which must be taken into account when making aesthetic judgements about tonal quality. And the subdivision of factors involved must be carried further. Thus the element of dominance of formant regions in musical tones can be fractionated so as to determine what qualitative characteristics of the tone are due to specific types of distribution of formant regions. This is now illustrated in acoustical studies of speech where vowels are defined in terms of characteristic formant regions. This method is now employed in comparative studies of musical instruments. Progress is slow but in firm step so that any one who teaches, thinks, or creates intelligently in terms of tonal quality should understand the structure and function of formants. Composers are becoming alarmed at their growing responsibility for formants, both objective and subjective.

V. THE METHOD OF NATURAL SCIENCES IS APPLICABLE IN MUSICAL AESTHETICS

In a virgin territory, the botanist collects as many and varied specimens as he can find and then takes them into his laboratory, examines each in detail, and names and classifies them. So the student of aesthetics may collect specimen features of beauty or ugliness in actual musical situations, and examine, name and classify them. Take again our problem of timbre. Each specimen is represented by a spectrum. No botanist has collected and classified all plants, but at one time botanists began to do so and acquired confidence in their ability. No one will ever collect samples of all varieties of beauty in the quality of musical tones, but each investigator and each generation will go as far as possible under the limitations of time and the extent of facilities. Preceding science has given him the tools, such as the art of determining the spectrum and the insight into the nature and ramifications of its significance.

VI. THE METHOD OF PHYSICAL SCIENCES IS ALSO APPLICABLE IN MUSICAL AESTHETICS

The sustained illustration which I am carrying through is an example of the use of physical method in so far as we are dealing with the nature and significance of musical sounds. We can not only make physical analysis of specimen sounds taken from actual music, but we can reverse the process and build synthetic tones on the specifications for any harmonic structure. We now have in the laboratory a tone generator capable of sounding tones of any desired harmonic structure composed of the first sixteen partials or less. The operator at the panel can determine how many partials shall be present, the character of their distribution, the amount of energy to be assigned to each, and the phase relationships of partials. Then he presses the button and out comes the tone of the structure specified. With so many variables, the mathematician tells us we could produce more than a million varieties of tone, thus imitating voice, musical instruments, sounds in nature or any other theoretically desired kind of tone. Thus we proceed in the opposite direction of the naturalist and produce tones synthetically in order to determine the range and types of recognizable and significant elements of musical beauty.

VII. SCIENCE IN AESTHETICS PROCEEDS BY THE OPERATIONAL METHOD

As a sequel to the fractionating of problems for the purpose of the experiment, psychologists profitably take an operational point of view; that is, they state the circumstances under which the observation was made in such a way that the conditions can be repeated, and they limit their primary conclusions to those factors which were actually under control. The first step is to show what the actual experiment is in this particular case. If we are then interested in whether or not that is regarded as a relatively good or poor spectrum, we would have to resort to further experiment with aesthetic judgements. If we want to generalize, we would have to make a corresponding series of measurements on the principal variables which may enter; and if we want to study the variability in the aesthetic value of this particular tone, we would have to experiment with some of the principal variables such as age, training, artistic temperament, or what the observer had for breakfast. In other words, the progress of experimental aesthetics is exceedingly slow and is limited primarily to the establishment or verification of basic facts.

VIII. SCIENCE ENCOURAGES CO-OPERATION WITH ALL OTHER LEGITIMATE APPROACHES

Under the preceding headlines I have attempted to state the point of view represented by a psychologist in musical aesthetics in his attempt to interpret scientific approaches. Other experimenters in this field might and do differ from this point of view, but I submit it as a fair sample in the light of which the validity of the assumptions and criticisms of Mainwaring might be reviewed.

There are many problems in aesthetics which should be approached mainly from the philosophical point of view, such as theories of aesthetic value or the nature of beauty as reality. One must also take cognizance of the inspirations of the mystic. The splendid progress made in creative music has furnished the most basic groundwork we have for the determination of aesthetic values, particularly as dealing with problems and possibilities of design in musical form. The artist's interpretation of the printed score is, of course, the commonest object of aesthetic judgement. Musical anthropology envisages the racial development of art principles. Musical history is a critique of unfolding art principles. Genetics traces the rise of aesthetic judgements in the growth of the individual. Education develops methods for training in aesthetic judgement. The experimental sciences unravel new facts and can put existing theories and practices to the acid test. The pursuit of beauty is fascinating and profitable at each of these fronts.

Why not encourage all such approaches? Let the musical historians, anthropologists and critics enlarge their storehouses and submit to periodic house-cleaning by separating the valid material from the invalid. Let the composer enlarge our conceptions of beauty by adding to our heritage of musical creations as art objects. Let the educationist put evolving theory into practice. Let the philosopher tell us from time to time what the greatest thinkers have thought about the nature of beauty and aesthetic values. Let the experimenter be ever ready and challenge all comers to verify and validate their facts and theories. Let us clarify the concept of beauty progressively by requiring each contributor to define beauty from his area, his purpose and his point of view.

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THE APPRECIATION OF HUMOUR: AN EXPERIMENTAL AND THEORETICAL STUDY[1]

By H. J. EYSENCK

From the Psychological Laboratory, University College, London

I. *Introduction* (pp. 295–296).
II. *Plan of investigation* (pp. 296–297).
III. *Experimental results* (pp. 297–301).
IV. *Temperamental factors* (pp. 301–302).
V. *Theories of humour* (pp. 302–307).
VI. *Summary and conclusions* (pp. 307–308).
References (pp. 308–309).

I. INTRODUCTION

IN the investigation reported here, an effort was made to deal with three questions in the field of humour by means of the method of factorial analysis. The first of these questions concerns the relative generality (or objectivity) of the appreciation of humour. Some investigators, such as Heim (2), report little or no agreement among the subjects taking part in their experiments with regard to the 'funniness' of the jokes used as stimuli; others, such as Stump (3) and the authors of the Roback, Moss, and Allcock tests of humour, explicitly or implicitly assume a comparatively high degree of agreement. No quantitative answer, stating the actual amount of agreement with reasonable exactitude, would appear to have been given to date.

The second question relates to the different types of appreciation involved in responses to humorous stimuli. While a great number of such types have been suggested by various authors, others have maintained that any attempt to discover such types "would merely involve a listing of individual jokes and individual people" ((2), p. 161). Here also, a quantitative answer showing clearly the relative importance and influence of any type factors would appear desirable.

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[1] This paper reports part of the work carried out by the writer during his tenure of the John Stuart Mill Research Scholarship at University College, London. Several researches of a similar nature have been, and are being, carried out at University College, and the writer wishes to record his indebtedness to Prof. C. Burt for valuable suggestions and criticisms. Acknowledgement is also due to Dr H. Babington-Smith for her kindness in giving the tests described below to a number of Scottish subjects, and to Prof. C. Landis, who very kindly allowed the writer to use his original material in this investigation, and who answered several queries relating to his own experiment (1).

The third question to be raised concerns the influence of temperamental factors on the appreciation of humour; more particularly, it appeared possible that the 'Personality Factors' isolated by Guilford (15) might to some extent determine the subjects' reactions to the various tests. An investigation of this possibility constitutes the third part of our experimental study.

Lastly, the experimental results gathered in the course of the investigation, and the subjects' comments and introspections, appeared to throw a certain amount of light on the vexed problem of the theory of humour and laughter, and it seemed worth while to record briefly the main conclusions to which the data seemed to point.

II. PLAN OF INVESTIGATION

Three humour tests, containing respectively 100, 52, and 37 items, were given to a group of sixteen subjects, the instructions being in each case to rank the items in each of the tests in order of 'funniness'. Eight of the subjects were University students; the other eight were entirely unconnected with academic life. The distribution of the sexes was equal. Ages ranged from 17 to 35. Introspections of the subjects were in some cases taken down in shorthand, while in other cases the subjects themselves wrote down valuable comments. The number of items in each of the three tests was too large to make straightforward ranking possible, and a group-ranking procedure had to be employed. By this method, marks are given to the various items in accordance with a prearranged system, the same mark being given to a fixed number of items. The distributions used followed as closely as possible the normal probability curve.

The tests were chosen with a view to covering three comparatively distinct varieties of humour, viz. verbal jokes (to be called 'jokes'), humorous drawings with captions (to be called 'pictures'), and the humour resulting from the opposition of two photographs showing certain points of similarity while being entirely different in other respects (to be called 'comparisons'). As an example of this last type of humour, the opposition of a photograph of Laval to one of a toad may be quoted, where there is a notable similarity of attitude and expression. Collectively, the items in the three tests will be called *jokes*.

The verbal jokes used were identical with those used by Landis & Ross in their investigation (1); their selection was carefully made so as to contain roughly equal proportions of good, medium and bad jokes, and to contain representative examples of the seven types of humour recognized by Landis and Ross in accordance with the theoretical analysis of 'sense of humour' given by Eastman (4).[1] These seven types of humour are defined as follows:

(1) *Humour of Quantity.* Results primarily from obvious exaggeration (over- or under-statement of facts, thoughts, etc.).

(2) *Humour of Incongruity.* Results primarily from the association of two generally accepted incompatibles.

(3) *Humour of Unexpected.* Results primarily from the occurrence of some surprising fact, thought, feeling, etc.

(4) *Humour of Truth.* Results primarily from projection of self into situation, with consequent exposure of unrevealed thoughts.

(5) *Humour of Superiority.* Results primarily from the inability of others to handle adequately situations which to us seem simple.

(6) *Humour of Repression.* Results primarily from the release of tension aroused by thoughts, feelings, etc. of sex, fear, etc.

(7) *Humour of Ridiculous.* Results primarily from obviously nonsensical use of logic, verse, etc.

The 'pictures' were selected by the present writer on the same principles from several thousand drawings which had appeared in *Punch, Lilliput, Everybody's, Illustrated, Razzle, The Humorist, College Humor,* and *Movie Humor.* The 'comparisons' were taken exclusively from the pages of *Lilliput,* and an effort was made to include examples of all the different types of 'comparisons' appearing in that journal.

Two controls, or 'jokers', were introduced into the 'pictures' and the 'comparisons' tests in order to test certain assumptions regarding the influence of the behavioural field. These controls, while similar in form to the other items in the tests, were made up in such a way that they were not funny at all, but quite meaningless: in the 'pictures' test, the captions were cut off two drawings, and replaced by quite unrelated captions, and in the 'comparisons' test two comparisons were made up by putting together entirely unrelated photographs.

III. EXPERIMENTAL RESULTS

In Table 1 are given some of the numerical results of these experiments. Two statistically significant factors were extracted from the correlations between the rankings of the items in each of the three tests;

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[1] This analysis is quoted below in some detail because it is typical of many other attempts. The present writer is dubious about its value as a psychological account, but it would appear that in the absence of a better analysis it may be of help in making sure that no important type of humour is overlooked in the selection of jokes.

from the 'pictures' test a third factor was extracted whose significance is doubtful. The numbers of correlations exceeding Fisher's $p=0{\cdot}05$ and $p=0{\cdot}01$ levels in each of the tests are shown below as 'significant correlations' and as 'very significant correlations' respectively.

Table 1

	Jokes	Pictures	Comparisons
Average intercorrelation	0·14	0·20	0·15
First factor variance	16·5 %	22·1 %	19·6 %
Second factor variance	6·2 %	5·6 %	7·0 %
Third factor variance	—	5·2 %	—
Significant correlations	32	34	30
Very significant correlations	18	17	9
Highest $+r$ in table	0·37	0·55	0·58
Highest $-r$ in table	0·17	0·08	0·45
Percentage of jokes found funny	35	44	51

These results enable us to answer our first query: How much generality or objectivity is there in each of the three tests employed, for the sample of the population tested? The answer is given by the percentage contributed to the variance by the first, positive, general factor: 16·5 % in the 'jokes' test, 22·1 % in the 'pictures' test, and 19·6 % in the 'comparisons' test. As these values are comparatively close together, we can take their average, 19·4 %, as representative. This value is very similar to the average of the first factor variances for eighteen tests of aesthetic appreciation reported by the present writer (5), which reached the value of 19·9 %. The average of the intercorrelations in these eighteen aesthetic tests was 0·171, while in the three humour tests it was 0·163. Thus agreement on the relative 'beauty' of the 280 pictures used altogether in the aesthetics investigation was almost identical in degree with the amount of agreement found in this experiment with regard to the relative 'funniness' of 189 jokes.

In the aesthetics experiment just mentioned, it was found that those observers who had high saturations in one test tended to have high saturations in other tests also. The correlations between the subjects' first factor saturations for the three humour tests used in the present investigation were found to be all positive, but not statistically significant. (According to Fisher's test for small samples, a correlation of 0·50 would be significant ($p=0{\cdot}05$) and a correlation of 0·62 would be very significant ($p=0{\cdot}01$) when the number of items correlated is 16.) The actual correlations are 0·17 ('jokes' and 'pictures'), 0·26 ('jokes' and 'comparisons'), and 0·05 ('pictures' and 'comparisons'); the average of these correlations is 0·16.

There is a marked tendency for the person who finds a large number of jokes funny in one test to find a large number of jokes funny in other tests also. The correlations between the tests are 0·66, 0·58, and 0·41, averaging 0·55. While it is probable that these correlations are to some extent due simply to different 'levels of aspiration' in the subjects, the introspections show that there is quite a genuine difference between them as regards the number of jokes found at all amusing. There is a slight but constant positive correlation between liking a large number of jokes and having a high factor saturation for the general factor; the correlations are 0·11 ('jokes'), 0·47 ('pictures'), and 0·08 ('comparisons'), averaging 0·22.

As regards the question of type factors in the appreciation of jokes, the most definite and clear-cut of these factors is found in the 'pictures' test. This bipolar factor, which contributes 5·6 % to the variance, divides those observers who show a certain preference for sexual jokes from those who put jokes of this kind comparatively low in their rankings. Neither sex nor age seemed to have any very obvious influence with regard to this factor. No sexual factor occurred in the 'comparisons' test because the material used contained no sexual jokes. In the 'jokes' test, where a few of the items were slightly sexual, a third factor extracted from insignificant residuals seemed to indicate a similar dichotomy between liking for sexual jokes, and dislike for them, but the dichotomy was not marked enough to permit of definite interpretation.

Interesting defence mechanisms appeared in some cases when subjects who did not like sexual jokes offered some far-fetched interpretations of the joke which left out its obvious sexual significance, or just refused to understand it at all. This appears to be in good agreement with Ghosh's finding that "in some cases, factors have been overstressed or even read into jokes, while other factors were neglected or overlooked in order that the jokes might be made to fit into the pattern of the subjects' mental states" (6). While this misunderstanding of jokes did not occur exclusively where sexual jokes were concerned, it seemed to occur most frequently in these cases. Sexual jokes seem to fall largely into Eastman's categories of 'Humour of Truth' and of 'Humour of Repression'.

The third, bipolar factor extracted from the 'pictures' test indicates an opposition between one group of subjects who prefer jokes which are based mainly on the *character* of the persons depicted (personal aspect), while another group prefer jokes which are based mainly on the humour of the *situation* (impersonal aspect). This factor seems to be closely related to the personal-impersonal factor found by Kambouropoulou in her analysis of diary entries, joke tests, etc. (7). One might feel tempted to relate this factor to the familiar opposition between formal and representative art; the impersonal type of humour is created by the formal aspects of the situation, while the personal type depends on the representative character of the people depicted.

The second, bipolar factor in the 'comparisons' test, which contributed 7·0 % to the variance, opposes one group of people who tend to prefer photo-comparisons in which one of the two pictures is amusing by itself, without taking into account the relation with the other picture, to another group who place great stress on this relation, without paying much attention to the amusing nature of the individual pictures. We may in this connexion recall a similar factor found in the preference rankings of colour combinations; some people judge almost entirely by the individual colours in the combination, while others base their judgement largely on the relations between the colours (8, 9). This opposition between the group who preferred the complex, relational 'comparisons' and those who preferred the simpler ones became apparent also in their comments; the 'comparisons' depending for their effectiveness on the relation between the pictures were often called 'clever', while the other comparisons tended to be called 'funny'. Martin, in her pioneer work, noted the same opposition ((10), p. 39), and it seems to have played an important part also in the distinction drawn by Hollingworth between 'waxing' and 'waning' jokes (11).

The second, bipolar factor in the 'jokes' test, which contributed 6·2 % to the variance, appears to be related in some measure to the factor discussed above. Here also one group of subjects like the simple, straightforward, 'funny' type of joke, while the others prefer the more complex, 'clever' sort of humour, in which a comparatively large number of relations requires to be educed. It is interesting to note that a similar difference has been found to play an important part in aesthetic preference judgements: a complexity-simplicity factor was found in work on preferences for poetry (12) and for polygonal figures (13).

The controls or 'jokers', mixed in with the ordinary 'pictures' and 'comparisons', occupy on the average the last two places in the ranking orders. Several subjects, however, put them relatively high in their rankings, in two cases awarding them next to the maximum number of points. This shows how easy it is to create artificially the desired ego-object relation, and how efficient an artificially created relation of this kind can be in changing the behavioural field of the subjects.

There was no differentiation of the sexes in any of the factors discussed above. This result is in good agreement with Heim's finding, who discovered "a surprisingly high degree of agreement between men and women" (12), p. 155). She discounted her own finding as "probably untrustworthy", giving no reason beyond saying that "everyone undoubtedly believes that fundamental and universal differences exist" (ibid.). In the absence of proof for the existence of such differences, it would appear safer to rely on the experimental results, instead of on popular conviction.

In view of the fact that the number of subjects taking part in this investigation was relatively small, the question may be raised of just how much reliance can be placed on the results. Some aspects of this problem have been discussed elsewhere (14), and when the formulae quoted there are applied to the results summarized above it is found that the correlation of the average order given by our subjects for the items in the three tests would correlate with the true order of the whole population of which they are a sample to the extent of approximately 0·90. High values of this kind would appear to lend a certain degree of validity to our conclusions.

IV. TEMPERAMENTAL FACTORS

In an attempt to obtain a picture of the subjects' temperamental peculiarities, they were asked to rank in order of 'applicability' twenty-five temperamental traits, typed on separate slips of paper. That is to say, they were instructed to put the trait which they considered most characteristic of themselves at the top, the one they considered least characteristic at the bottom, and the others in between in order of applicability. The traits were taken from Guilford's research into personality factors *S*, *E*, and *M* (15), by choosing the traits with the highest saturations for these three factors.

The rankings of these traits were then correlated, and the table of intercorrelations factor-analysed. Two factors, both of which were significant, were extracted, accounting for 30·0 and 11·1 % of the variance respectively. Both factors were bipolar, and while the first factor seemed to differentiate between the aggressive, masculine type of person, and the emotionally dependent type, the second factor distinguished between the sociable and the unsociable. In other words, factor I opposed Guilford's traits *M* and *E*, while factor II opposed the two aspects of his trait *S*.

Several correlations of these two temperamental factors and the type factors isolated earlier in this study approached or reached statistical significance. The sexual factor correlated to the extent of $-0{\cdot}48$ with the emotionally dependent-aggressive factor, and to the extent of $-0{\cdot}42$ with social shyness. This connexion between the liking for sexual jokes and an aggressive, not emotionally dependent, sociable kind of temperament is perhaps not contrary to what one would have expected on a priori grounds, and is of particular interest in view of Ghosh's findings (6). It may also be noted as giving support to certain orectic theories of humour and laughter which will be discussed in the next section.

The complexity factor in the 'jokes' test was found to correlate 0·16 with the emotionally dependent-aggressive factor, and 0·61 with the factor of social shyness. The complexity factor in the 'comparisons' test correlated 0·21 with the emotionally dependent-aggressive factor, and 0·21 with the factor of social shyness. As both social shyness and emotional dependence are generally regarded as introverted characteristics, while sociability and aggressiveness are extraverted characteristics, we are perhaps justified in the conclusion that introverts tend to prefer complex jokes, while extraverts tend to prefer simple jokes. (Similarly, introverts tend to dislike sexual jokes, while extraverts tend to like them.) While the correlations on which these conclusions are based are not always significant, it should be noted that they all point in the same direction.

It may be of interest to note here that the connexion between introversion and liking for more complex stimuli does not seem to be restricted to the field of humour. In an investigation into the factors determining the appreciation of poetry, a similar connexion has been noted (12), and from the references quoted there it will be seen that a similar relation between temperament and aesthetic preferences obtains also in other fields.

The correlations between the personal-impersonal factor, and emotional dependence and social shyness, are too low to be of any significance, being only 0·05 and 0·15 respectively.

While the conclusions stated above are suggestive rather than definitely established, it seems certain that temperamental traits influence the appreciation of jokes, just as they have been shown to influence the appreciation of poems, pictures, etc. It would appear likely that further experimentation involving other personality factors would throw considerable light on the genesis of laughter.

V. THEORIES OF HUMOUR

When the theories of humour advanced by the one hundred or so best-known authors are examined, little agreement is found on any of even the most fundamental points. This disagreement is due partly to the fact that most authors in the past have attempted to relate their theories of humour to their general philosophical theories, thus introducing into the former many of the questionable assumptions and controversial views of the latter; it is due also to the fact that few philosophers have given much attention to the fundamental question of exactly what are the stimuli which cause laughter.

Again and again, the hat that is blown off by the wind, or the fall in the street, are quoted as typical mirth-provoking instances; thus Bergson begins a famous passage in his book by saying: "A man, running along the street, stumbles and falls; the passers-by burst out laughing" (16). A numerical count by the present writer of seven occasions when four men and three women stumbled and fell, and of five occasions when the wind blew off the hats of two men and three women, did not reveal a single instance of laughter among the total of more than 100 passers-by. Facts such as these must make us suspicious of all theories which might explain after a fashion why laughter should occur, but which fail to explain why actually it does not occur. Similarly, philosophical theorists frequently posit certain feelings of superiority or of expectation which those who laugh are supposed to experience in certain circumstances, but which careful introspection often fails to discover.

When the actual theories which have been suggested to date are surveyed, we find such a variety that it is difficult to discover an underlying basis according to which they might be classified. To a psychologist, the most satisfactory classification is perhaps one which follows the usual division into cognition, conation, and affection, and indeed most theories seem to fall quite naturally at some point in a triangle whose three corners are marked as in Fig. 1.

Most numerous of all are those theories which stress such cognitive elements as incongruity, contrast between ideas, deceived ideational expectation, and the like. The long list of writers who have held such theories contains among others the names of Cicero, Quintilian, Dryden, Locke, Marmontel, Gerard, Campbell, Beattie, Priestley, Kant, Jean Paul, Hazlitt, Brown, Schopenhauer, Everett, and those who, following Spencer, introduced the added requirement that the incongruity should be descending—Lipps, Sidis, Marshall, and Renouvier and Prat. Schiller and Willmann may be quoted as modern champions of some form of cognitive theory, and so may Maier.

Almost equally numerous is another group of writers who stress the conative aspect of laughter, relating it to the satisfaction of the desire for superiority, or 'self-glory' as Hobbes has it. In this class are, for instance, Plato, Aristotle, Trissino, Hobbes, Hegel, Lamenais, Hunt, Bain, Philbert, Michiels, Carus, and Bergson. Chandler (17) and Kimmins (18) have provided a certain amount of experimental proof in favour of this theory, which finds its latest champion in Ludovici (19). Following Wrench (20), he has suggested the term 'superior adaptation' as characterizing all instances of laughter, such laughter in his view being due to the consciousness of superior adaptation on the part of the person laughing.

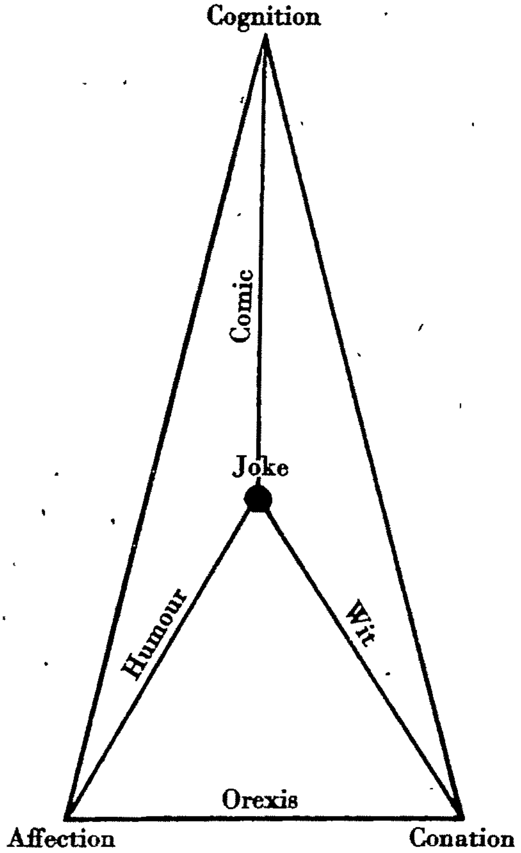

Fig. 1. Diagram representing the structure of the joke, showing the three-fold determination of laughter by cognitive, conative, and affective factors.

The affective aspect of laughter is stressed by those who have directed their attention more to the emotional component of laughter. This is usually conceived to be pure joy, or else joy in combination with some other emotion, such as fear or anger. Alternatively, a contrast of feeling is posited as being essential to laughter. Joubert, Descartes, Hartley, Laprade, Dumont, Höffding, and McDougall may be mentioned here.

Some writers cannot be placed near one corner of our triangle, but must find their place along one side; thus Ribot, Sully, and Santayana have advanced theories which recognize both the cognitive and the conative aspects of humour. Freud may be said to have recognized all three aspects to some extent; his definition of wit as being due to an economy of expenditure in inhibition stresses the conative aspect, his definition of the comic as being due to an economy of thought stresses the cognitive aspect, and his definition of humour as being due to an economy of feeling stresses the affective aspect. His theory is vitiated, as Eastman points out, by his uncritical acceptance of the mechanical Spencer-Lipps theory of 'economy', which is really foreign to the remainder of his views. He may also be criticized for making too rigid a distinction between the three different kinds of laughter, i.e. that of wit, of humour, and of the comic; the mind acts as a whole, and in every case of laughter all three components must play their part, although their relative importance may vary from case to case.

In considering these three divisions, it appears that we can with advantage group together the conative and the affective aspects, under the general name of 'orectic'. The reason for this grouping is of course that these two aspects are related much more closely to each other than either is to the cognitive aspect. If we follow Ludovici in his well-documented account, we may define the orectic component of laughter as being due to the 'joyful consciousness of superior adaptation'. We cannot follow that author, however, in neglecting entirely the cognitive aspect, particularly as it is this component which is most noticeable in the type of material used in the present investigation. Before turning to an analysis of this cognitive component, we may note certain facts regarding the relative influence and importance of the orectic and cognitive factors.

Ludovici's own data seem to show that historically and genetically we have a progress from the laughter of superior adaptation, as shown for instance in the laughter at physical imperfections and at accidents, to the laughter at the intellectual joke, which is almost purely cognitive (19), pp. 88–103). Even in the realm of the joke, however, we find the opposition between the orectic and the cognitive aspects; the contrast between 'funny' and 'clever' jokes, noted in a previous section, appears due to the relative preponderance of orectic or cognitive factors. That this is a true interpretation is indicated by the correlation between aggressiveness and liking for simple, 'funny' jokes. The correlation between liking for sexual jokes and aggressiveness seems also to be due to the orectic factor. These results seem to indicate that the view often held, viz. that introverts have less sense of humour than extraverts, is false, and that we should say, rather, that introverts have an appreciation of humour in some ways different from that of the extraverts. More particularly, while in extraverted persons the orectic aspect of humour is paramount, in introverted persons the cognitive aspect is supreme.[1]

While apart from the correlations just mentioned there is little in the results of this research to help in a discussion of the orectic components of laughter, our material is more extensive with regard to the cognitive aspect. When the experimental material is examined with particular reference to the 'funniness' of the items, as revealed by their average position, and to the introspections and comments of the subjects, certain conclusions emerge which seem to be helpful in an analysis of the cognitive factor. The majority of the points noted are not new, and have indeed recently been discussed by Maier (21) and Willmann (22) in their interesting theoretical contributions, but in one or two places there are certain amplifications and modifications which appeared to be essential in view of the experimental results.

(1) In the first place, we find that each one of the jokes used contains two or more ideas, attitudes, or sentiments between which there is a certain amount of *contradiction* or *incongruity*.

(2) Secondly, we find that these contradictory ideas, attitudes, or sentiments are fused, united, or *integrated* to a certain extent during the course of the joke, and that laughter results when this point is reached.

(3) Thirdly, this fusion, union, or integration is *sudden*; when it is protracted, the joke arouses no laughter and loses its point.

(4) Fourthly, the process of integration is accompanied by *insight*; as Maier says, there is a change in the meaning of the elements of the humorous experience. The total configuration of ideas, attitudes, or sentiments built up in the first part of the joke is changed, and we gain an insight into the structure of the total field which previously was lacking.

(5) Fifthly, the elements contained in the joke must be experienced

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[1] In support of this view, it may be noted that none of the correlations between the temperamental factors and the first, general, positive factor in the humour tests even approached significance. On the customary view, the extraverted traits ought to be correlated positively with this general, positive factor. The complete absence of any such correlation favours the view outlined above.

objectively, not emotionally. (This demand, of course, is the counterpart to the demand for 'psychical distancing' in aesthetics.) This condition does not eliminate the occurrence of an emotion of joy as a result of the joke, but merely prevents any emotional attachment to the elements contained in the joke.

Taking these five points together, we may say that on the cognitive side, *laughter results from the sudden, insightful integration of contradictory or incongruous ideas, attitudes, or sentiments which are experienced objectively*. Other things being equal, the funniness of a joke is a direct function of the degree of contradiction or incongruity between the main ideas, attitudes, or sentiments contained in it, and the quality of the integration of these elements, as measured by the suddenness of, and the degree of insight resulting from, this integration.

While it is permissible for theoretical purposes to isolate the cognitive aspect of humour in this fashion, it should not be forgotten that in each particular case of laughter the orectic aspect too must be considered, even though in experiments of the kind described here the influence of this factor is reduced to a minimum. In general, there will be little difficulty in deciding in each case of laughter where in our triangle its main cause ought to be looked for, and the usefulness of such an eclectic theory as that presented here will not be diminished because it may prove impossible to find many causes of laughter which can be placed exactly at one corner of the triangle; it seems obvious that the great majority will be found somewhere inside the figure, the exact position being determined by the relative importance of the three factors we have distinguished.

VI. SUMMARY AND CONCLUSIONS

Three tests involving the ranking of altogether 189 jokes of various kinds were given to sixteen subjects. The resulting rankings were correlated, and the tables of correlations factor analysed. A test of temperament was also given.

In each of the three analyses, a positive, general factor appeared first, accounting on the average for 19·4 % of the variance. Several bipolar factors were also extracted, each of which contributed some 5–7 % to the variance. These factors divided the subjects into types according to the following principles of classification: (1) liking for *sexual* as opposed to *non-sexual* jokes; (2) liking for *complex* as opposed to *simple* jokes; and (3) liking for *personal* as opposed to *impersonal* jokes.

Extraverts were found to prefer sexual and simple jokes, while introverts preferred complex and non-sexual jokes. It was suggested that the assertion often made which attributes a better sense of humour to the extraverts than to the introverts is wrong, and that we are dealing rather with differences in the manner of appreciating humour than with differences in the amount of sense of humour possessed by the two types.

Analysis of the jokes used, their relative positions in the average ranking order, the introspections and comments of the subjects and the various correlations between types of appreciation and temperamental factors, led to a theory of humour which stressed the complex nature of the phenomena investigated. On the orectic side, the results supported the view that laughter was due to the joyful consciousness of superior adaptation, while on the cognitive side the conditions responsible for the emergence of laughter could be summarized under five headings which emphasized the sudden, insightful integration of contradictory or incongruous ideas, attitudes, or sentiments which are experienced objectively.

The distinction between the orectic and the cognitive aspects of laughter was declared to be useful in theory, but the warning was added that in practice *both* factors are usually active, in varying proportions, and that therefore the distinction must not be carried too far.

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(*Note.* No detailed references are given for the classical writers whose names are quoted in this paper. Full references will be found in the works of Eastman (4), Greig (23), and Gregory (24), together with brief but usually adequate summaries of their views.)

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FOLLOWING UP INDIVIDUAL ITEMS IN A GROUP INTELLIGENCE TEST

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II. *The technique of item analysis* (pp. 311–312).
III. *The data* (pp. 312–314).
IV. *Discrimination between secondary school and intermediate school* (p. 315).
V. *Discrimination within the secondary school* (p. 316).
VI. *Discrimination within the intermediate school* (pp. 316–317).
VII. *Conclusion* (p. 317).

I. OBJECT OF THE ENQUIRY

THERE have been many researches in which the predictions of an entrance group intelligence test, taken as a whole, have been followed up during the careers of children in secondary schools or in junior secondary schools,[1] more often in the former than in the latter. Other researches have compared the predictive power of different sections of the group test, as for example the *analogies*, the *number series*, the *absurdities*, and have found regression weights for combining these to give the best correlation with future success. But there have not, as far as I know, been any enquiries (and if any, there certainly have not been many) which have followed up each of the individual items of the test. This is what is done in the present article. The object of course is to gain still more information with a view to producing still better tests. At the back of my mind was also the suspicion, which has been somewhat deepened by the results of this experiment, that it is not so much the type of the item which matters (as *analogy*, or *absurdity*) as the difficulty value of the individual item. The research also throws light on the fact that the predictive power of a test *within* the secondary school is not necessarily, indeed not usually, the best indication of its power to separate the potential secondary school pupils from those not suitable for (academic) secondary school courses.

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[1] Intermediate Schools, Central Schools, Advanced Divisions, are among the names given to these schools.

II. THE TECHNIQUE OF ITEM ANALYSIS

The technique to be used was already fully available, for it is the same as that used in selecting items to make a new test. To make a new test of 100 items we in Moray House devise 300 items, many of them similar to those of former tests, and give them (in suitable doses) to 300 children. These children are divided, by some means or other, into six equal groups, ranging from the cleverest to the dullest. Sometimes this can be done by asking the opinions of their elementary school teachers; sometimes by elementary school examination results; sometimes by an already established and trusted group intelligence test; most easily by the total results of the 300 new items themselves.[1]

For each of the 300 items it is then ascertained how many children in each of these separate groups have answered it correctly; and broadly speaking, those 100 items are retained which discriminate best between the clever and the dull. But, as will appear below, much depends on whether we want to discriminate between the very clever and the nearly-as-clever, or between the mediocre and the dull, or at some other point, or generally over the whole range.

Various methods of calculating coefficients of discrimination have been published, mostly in the American journals, and we employ some of these in our laboratory. But for the explanatory purposes of this article it is better I think merely to draw graphs and compare them by eye. For example, in Fig. 1 (p. 314) are shown the graphs for two items. The dots represent the six groups of children from clever to dull, and the left-hand scale represents the proportion answering an item correctly. It is clear that item 1 is easy. None of the best two groups of children failed to answer it. Only the poorest group failed in any substantial numbers. This item asked "What is the fifth letter of the alphabet?" Despite its simplicity and lack of discrimination except at the lowest level of ability, it was retained in the test as its first item as a 'warming-up' item intended to give confidence, and indeed really to act as part of the instructions. Item 24, on the other hand, as is evident at a glance, is much more difficult, and discriminates over nearly the whole range, especially in the middle portion where the slope of its graph is steepest. This item asked for the next number after 1, 2, 4, 8, 16.

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[1] This last plan suffers from the obvious disadvantage of being to some extent an argument in a circle, and I have for long desired to have some idea of how item selection by this means compares with actual predictive power, a question to which the present research, in conjunction with other data, gives an answer showing that this method is in fact not a bad one. But this point is not dealt with in the present paper.

It is clear that this technique can be immediately used in a follow-up. The plan is to have the children divided into groups according to the opinion of the post-primary schools to which they have gone (after they had been in those schools for one year, in our case) and draw such graphs for each item.

III. THE DATA

The test used was Moray House Test 24, and the children were 752 Durham County children of whom 259 had been in four secondary schools, and 493 had been in eight intermediate schools, for one year.

Moray House Test 24, after being made, in 1935, in the manner described in § II, was used privately by thirteen Education Authorities in various parts of England and Scotland, ten of whom administered it to a *complete* age group, as shown in Table 1. The test was then published (by the University of London Press, Ltd.) with a conversion table, based on these 30,022 cases, for turning raw scores into 'intelligence quotients', with centre at 100 and standard deviation of 15 points. In this form it has since been used in other areas, among them Durham County, where it was in 1940 given to 11,462 elementary school pupils aged 10–8 to 11–7 for the purpose of selecting those who might enter secondary schools or intermediate schools. The mean intelligence quotient, calculated by the above-mentioned conversion table, was in County Durham 102·5 for the boys and 102·3 for the girls. The excess above 100 is explained by the fact that the group of 11,462 tested was not the *complete* age group of 13,923. The 2461 who did not take the test were partly absentees,[1] but included a large number of very dull children who were not given the test because they were considered in any case too dull for an intermediate school. The same absence of the very dull probably also explains

Table 1

Code designation of area in Moray House records	No. of children	Age range
A	6,805	10–8 to 11–7
Y	4,476	10–8 „ 11–7
AE	3,430	10–2 „ 11–1
W	4,316	10–4 „ 11–3
D	818	10–0 „ 10–11
G	1,624	10–6 „ 11–5
AT	3,448	9–8 „ 10–7
AZ	1,872	10–5 „ 11–4
Q	1,006	10–6 „ 11–5
AF	2,227	10–8 „ 11–7
Total	30,022	

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[1] About 557 of the 2461, presumably children absent through illness, were later tested by another test.

the standard deviations in Durham County, of 14·4 for boys and 13·4 for girls (instead of 15 for a complete age group). Of the whole year group, 1458 then entered secondary schools and the rest entered either intermediate schools or senior departments. Four of the secondary schools (with 59, 62, 82 and 56 entrants respectively) and eight of the other schools (with 55, 42, 68, 50, 30, 40, 66 and 142 entrants respectively) were chosen for the follow-up of individual items of the test. Six of these eight schools were called 'intermediate', the other two (one for boys and one for girls) were not so designated. For simplicity, however, the group is in what follows called the 'intermediate' group. After the children had been for one year in these twelve schools the headmaster (or headmistress) divided those in his school into three groups, as nearly equal as possible, according to their suitability for the work of that school. The 259 secondary pupils were thus divided into three groups of 86, 87 and 86, called groups *a*, *b* and *c* in what follows, while the 493 intermediate pupils were divided into three groups of 164, 165 and 164, called *d*, *e* and *f*. Of course it must be remembered that the separation of the three groups *abc* from the three groups *def* was actually made in 1940 by the total test score. The separation of *a* from *b*, and *b* from *c*, however, and similarly of *d*, *e* and *f*, was done a year later by the teachers, regardless of the test score. The head teachers of the secondary schools (Miss Hilda Cowie, Messrs Wm. Carr, J. C. Cleminson, and T. Oates) then undertook the labour of ascertaining, separately for each of these six groups *a*, *b*, *c*, *d*, *e* and *f*, how many times each of the 100 items had been answered correctly. Sincere thanks are due to all the twelve head-teachers, and especially to these four, for their co-operation; and also to the Director of Education, Mr T. B. Tilley, for his interest and assistance. A diagram was then made for each item showing what proportion of each of the six categories had succeeded with the item (see, for example, Fig. 2).

The group of 259 secondary pupils were, it may be presumed, a fair sample of the whole 1458 who entered secondary schools throughout the county. The group of 493, however, who form the intermediate school sample are not a random sample of the unsuccessful, but a somewhat superior sample of them. The intelligence quotients corresponding to the average raw scores of the six groups were: *a*, 129; *b*, 126; *c*, 120; *d*, 111; *e*, 103; and *f*, 93; for *abc* together, 125; for *def* together, 102. The average I.Q. of all the children unsuccessful in entering secondary schools would be something below 100. Probably our 493 cases contain few representatives of the bottom twenty per cent (intellectually) of the children of the county. One of the intermediate schools on our list is 'selective'. The others are 'non-selective', but there has clearly been a certain amount of selection exerted not by authority but by circumstances.

Fig. 1

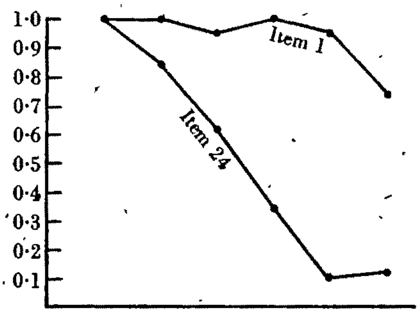


Fig. 2

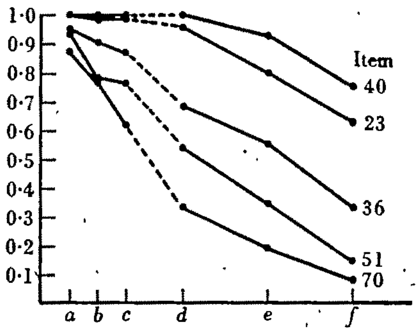


Fig. 3 (Item 68)

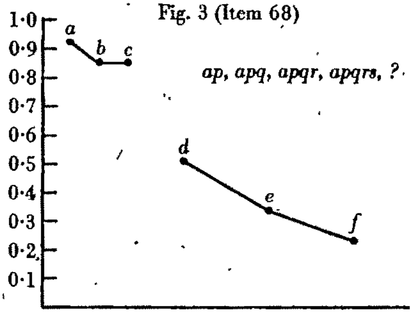


Fig. 4 (Item 70)

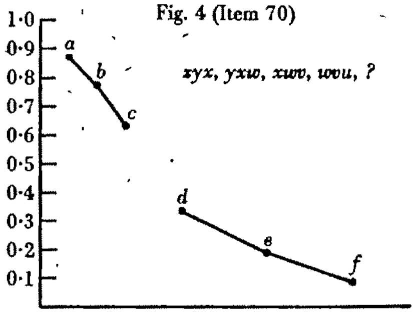


Fig. 5 (Item 90)

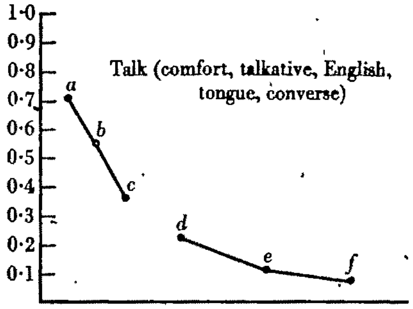


Fig. 6 (Item 60)

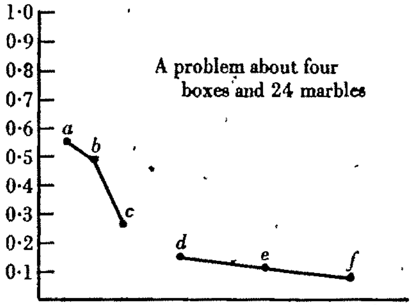


Fig. 7 (Item 28)

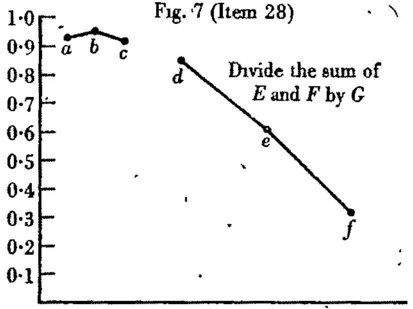
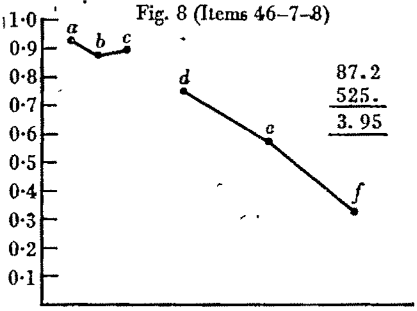


Fig. 8 (Items 46–7–8)



In these diagrams the points *a*, *b* and *c* refer to 259 secondary school pupils classified by their teachers after one year in the school, and *d*, *e* and *f* refer similarly to 493 intermediate school pupils. The ordinate of each point gives the proportion of that group who were successful in the item to which the diagram refers, in an intelligence test given before entrance to the school. The abscissae are not metrical, though the points are very roughly spaced, horizontally, as they might be expected to lie. Fig. 1 refers to a different group of children, a complete age group divided into six equal parts.

IV. DISCRIMINATION BETWEEN SECONDARY SCHOOL AND INTERMEDIATE SCHOOL

The hundred diagrams thus made for the separate items show at a glance not only that the items are of different degrees of efficiency, but that they perform different functions (see Fig. 2). The most important function of the test was, I think, to separate from the rest those pupils who were suitable for an academic secondary school education. Those items were presumably most successful in doing this which on the diagrams show the widest gap between the point *c* (the bottom third of the secondary school a year later) and the point *d* (the top third of the intermediate school, also a year later). The widest gap of this sort was shown by item 68 (see Fig. 3) which asked for the next group of letters in the series ap, apq, apqr, apqrs; where the gap is 0·349. But this may partly be due to the somewhat anomalous position of the point *c*. The second widest gap between *c* and *d* was in item 70 (see Fig. 4) which asked for the next member of the series zyx, yxw, xwv, wvu. Among the twelve items in which the gap between *c* and *d* exceeded 0·200 (see Table 2) there were four of this type, and two others also concerned with groups of letters, in which the child was asked to underline (italicized in table) the 'different' group which should not be there.

Table 2. *Items discriminating best between the bottom of the secondary school and the top of the intermediate school*

Serial no.	Short clue to nature of item*	Point *c*	Point *d*	Difference
6	Different: 5, *3*, 8, 28, 2	0·814	0·591	0·223
9	Different: 545, 767, 878, *313*, 656	0·837	0·585	0·252
51	conceal (obey, unseen, hide, comfort, find)	0·768	0·543	0·225
59	How many marbles in box *C*?	0·616	0·384	0·232
67	ll, mm, nn, oo, pp, next?	0·930	0·671	0·259
68	ap, apq, apqr, apqrs, next?	0·849	0·500	0·349
69	abc, bcd, cde, def, efg, next?	0·733	0·463	0·270
70	zyx, yxw, xwv, wvu, next?	0·616	0·335	0·281
71	up is to down as climb is to ?	0·779	0·512	0·267
79	Different: ponm, *decb*, dcba, xwvu, kjih	0·419	0·177	0·242
80	Different: ceg, moq, ikm, *efh*, tvx	0·477	0·220	0·257
96	Rings and knocks on different days	0·674	0·445	0·229

* The actual items were longer and preceded by explanation.

The reader will not have failed to note that the absolute height of the points *c* and *d*, and not merely their difference, is of importance. For example, items 9 and 79 have about the same gap between *c* and *d*: but item 9 would admit many, item 79 only few, to the secondary school. Item 79 would keep out many deserving candidates, item 9 would admit many undeserving.

V. DISCRIMINATION WITHIN THE SECONDARY SCHOOL

The items which discriminate best between the bottom of the secondary school and the top of the intermediate school are not usually the same as those which discriminate best *within* the secondary school, a fact which should give pause to those who judge an entrance test by the concordance between final order of merit in the secondary school and order of merit in the entrance test. The best discriminator within the secondary school, judged by the difference between the point *a* and the point *c*, was item 90 (see Fig. 5) which asked which of the words *comfort*, *talkative*, *English*, *tongue*, *converse* means nearly the same as the word talk. Item 60, the next best (see Fig. 6) says: "I have 24 marbles and four boxes. Leaving box *D* empty, I place twice as many in box *B* as in box *A*, and twice as many in box *C* as in box *B*, and have 3 left over. How many marbles are there in box *A*?"

Table 3. *Items discriminating best within the secondary school*

Serial no.	Short clue to nature of item*	Point *a*	Point *c*	Difference
5	Long shadows from N.W., what time?	0·547	0·279	0·268
30	Ear is to nose as hear is to ?	0·768	0·512	0·256
31	Penny is to sixpence as shilling is to ?	0·779	0·558	0·221
58	How many marbles in box *A*?	0·768	0·535	0·233
60	How many marbles in box *A*?	0·547	0·267	0·280
70	zyx, yxw, xwv, wvu, next?	0·872	0·616	0·256
75	Two letters of AMBLE same distance apart in alphabet	0·663	0·454	0·209
76	Two letters of GOLD same distance apart in alphabet	0·768	0·512	0·256
80	Different: ceg, moq, ikm, *efh*, tvx	0·698	0·477	0·221
89	Rest (repose, activity, oppress, restive, bed)	0·744	0·465	0·279
90	talk (comfort, talkative, English, tongue, converse)	0·709	0·360	0·349
93	Two sentences (of four) meaning nearly the same	0·919	0·709	0·210
94	Two sentences (of four) meaning nearly the same	0·605	0·326	0·279
95	Problem about rings and knocks on door	0·465	0·244	0·221
99	*Laundry* put into alphabetical order	0·837	0·581	0·256

* The actual items were longer and preceded by explanation.

The fifteen items of Table 3 are, with two exceptions, not included in Table 2.

VI. DISCRIMINATION WITHIN THE INTERMEDIATE SCHOOL

Similarly the ten items of Table 4 are different entirely from the items of Tables 2 and 3. Discrimination within the intermediate school is best performed by items other than those which differentiate at the other levels. Fig. 7 shows the curve for the item 28, and Fig. 8 for the average of the three items 46, 47 and 48. The 'differences' in this Table 4 between points *d* and *f* are larger than those in the former tables because there are more children in each group, and such selection as there is for the intermediate school is so much less stringent.

Table 4. *Items discriminating best within the intermediate school*

Serial no.	Short clue to nature of item*	Point *d*	Point *f*	Difference
8	Different: 55, 22, *45*, 88, 66	0·671	0·250	0·421
24	1, 2, 4, 8, 16, next ?	0·774	0·317	0·457
27	Divide D by G (number code)	0·780	0·360	0·420
28	Divide sum of E and F by G (number code)	0·854	0·311	0·543
29	What from F to get G? (number code)	0·799	0·348	0·451
(46)†	From 87.2	(0·616)	(0·232)	(0·384)
47	Take 525.	0·884	0·427	0·457
48	3.95	0·738	0·280	0·458
50	Hasten (chase, stop, hurry, has, Hastings)	0·890	0·482	0·408
62	Wear girls hats some brown	0·933	0·530	0·403
87	Latest letter (alphabetic) in *criterion*	0·732	0·329	0·403

* The actual items were longer and preceded by explanation.

† Item 46 is only included because of its close association with 47 and 48.

VII. CONCLUSION

It is hoped that the publication of this article, with its very simple if somewhat laborious technique, will encourage others to follow up individual items of tests of intelligence (and of other kinds of tests too). The labour is not great if team work can be made use of, and the method is therefore very suitable for co-operative research by the members of a teachers' association. The paper may also serve to increase confidence in tests by illustrating the care taken in their construction and later checking, and to throw light on the fact (not easily appreciated by non-mathematicians) that discrimination within a highly selected group may be small even though (or indeed just because) the instrument of selection has been efficient. Prediction correlations within a less highly selected body (such as, for example, the children entering a Scottish secondary school) are always larger than those within a more highly selected body (as, in comparison, the smaller entry to an English secondary school is).

(*Manuscript received* 18 *November* 1941)

PARTIALLING OUT SUMS OF SQUARES AND PRODUCTS IN CALCULATING CORRELATIONS WITH NON-HOMOGENEOUS DATA

By WILLIAM STEPHENS TAYLOR

I. *The importance of partialling out sums of squares and sums of products* (pp. 318–320).
II. *An example of the method* (pp. 320–322).
References (p. 322).
Note by a referee (p. 323).

I. THE IMPORTANCE OF PARTIALLING OUT SUMS OF SQUARES AND SUMS OF PRODUCTS

IN analysing psychological data it has been the normal practice to calculate correlations without attempting to partial out sums of squares and sums of products of deviations from the mean. Unless, however, one has independent evidence that the population tested is homogeneous, the values obtained may be misleading. They may consequently provide an unsatisfactory basis on which to build up factorial analyses.

In test results, or other quantitative psychological data, where the individuals tested fall into groups, the sums of squares of deviations about the mean are divisible into two parts—the sums of squares of the deviations of the group means about the grand mean, and the sums of squares of the deviations of the individual scores about their group means. In Fisher's (1) notation

$$S(y-\bar{y})^2 = Sn_p(\bar{y}_p-\bar{y})^2 - SS(y-\bar{y}_p)^2,$$

where $\bar{y}$ is the mean of all the values of y, the suffix p denotes any particular group, n_p the number in the group, and $\bar{y}_p$ the mean of the group. Sums of products of deviations about the mean can also be divided into the same two parts in the same way. With appropriate modifications the same formula can be used of them.

Where the population tested is homogeneous, the differences between the mean values of the various groups are insignificant. The sums of squares and sums of products of the deviations of group means about the grand mean will then be entirely negligible, and a coefficient of correlation, calculated in the ordinary way directly from the deviations of individual values about the grand mean, will give a reliable indication of the correlation between the scores of the individuals tested. Where the population is not homogeneous, however, group differences may be significant. Under these conditions, the value obtained by calculating the correlation coefficient in the ordinary way will be due to the averaging of an inter-class correlation between the means of the various groups and an intra-class correlation between the scores of the individuals composing the groups. The latter is the correlation desired, but its true value has been obscured by the inclusion of a value for the correlation of means. In order to obtain a true value for the correlation between the scores of the individuals composing the groups it is necessary to partial out the sums of squares (2) and sums of products of deviations from the mean, using only those attributable to the deviations 'within groups', i.e. to the deviations of the observations about their group means. The true value for the correlation between the scores of individuals, obtained after the sums of squares and sums of products have been partialled out in this way, may sometimes differ considerably from the values obtained in the ordinary way.

The following table of correlations (Table 1) illustrates this difference. In the upper half of the table are the correlation coefficients obtained in the usual way by calculating the deviations of all observations about the grand mean. In the lower half are the correlations obtained after partialling out sums of squares and sums of products. The coefficients in both parts of the table were obtained from the scores, on a battery of nine tests, of three successive classes of students in an Indian college. The students were divisible into two groups according to their native

Table 1

Test no.	Test no. 1	2	3	4	5	6	7	8	9
1		*0·3611	*0·3966	*0·3761	0·1840	0·0422	†0·2876	0·1296	*0·3800
2	0·0691		*0·4014	0·2025	0·2061	−0·0084	0·1994	0·1065	0·2239
3	†0·3264	*0·3703		*0·5161	†0·2850	−0·0503	0·0361	0·0995	†0·3326
4	0·2670	0·0492	*0·4294		†0·2966	−0·1479	−0·0120	−0·1962	*0·3780
5	0·2158	0·2071	†0·2865	†0·3098		0·0059	†0·3001	0·0318	*0·3589
6	0·2492	0·2313	0·0758	−0·0619	0·2190		†0·3374	†0·4333	−0·1002
7	*0·3686	0·2041	0·0465	−0·0071	0·2373	*0·5107		0·2003	0·1484
8	0·1360	0·1385	0·1384	−0·1992	0·0960	*0·4036	0·2505		−0·0949
9	0·1700	−0·0488	0·2524	0·2022	*0·3637	0·1212	0·1601	−0·0796	

* Significant on the 0·01 level. † Significant on the 0·05 level.

The tests used, and the numbers by which they are referred to, are as follows:

(1) Jumbled sentences test.
(2) Number series.
(3) Analogies.
(4) Absurdities.
(5) Sentence completion.
(6) Cancellation A.
(7) Cancellation P R S T.
(8) Retention.
(9) Logical memory.

language. In calculating the second set of coefficients, the sums of squares and sums of products for class differences, language differences, and their interactions were partialled out.

In many cases the corrected coefficients are considerably different from those obtained in the usual way: e.g. r_{12}, r_{14}, r_{16}, r_{17}, r_{19}, r_{24}, r_{26}, r_{29}, r_{34}, r_{49}, r_{56}, r_{67}. In cases such as these the uncorrected coefficients would be misleading.

There are a great variety of differences, of age, education, economic standing, language, social background, etc., which might introduce confusing group differences into an analysis of the scores of any given population. Under the best conditions, the meaning of the correlation coefficient is ambiguous. When the intra-class correlation which is desired is complicated by the admixture of inter-class correlations between means of groups it becomes even more ambiguous, and as a statistic may be definitely misleading. Unless one has independent evidence that the population tested is homogeneous, it is therefore important to partial out sums of squares and sums of products attributable to group differences before calculating the correlation coefficient.

II. AN EXAMPLE OF THE METHOD

The following hypothetical example[1] will serve to illustrate the method by which sums of squares and sums of products may be partialled out. A hypothetical example, with exactly ten students in each group, is given in Table 2 to simplify the calculations.

Partialling out sums of squares of deviations from the mean for test I

Total s.s. of deviations from the mean

$$= S(x-\bar{x})^2 = (Sx)^2 - \frac{S(x^2)}{N} = 28746 - \frac{994^2}{40} = 28746 - 24700{\cdot}9 = 4045{\cdot}1.$$

Total s.s. of deviations attributable to language differences, class differences, and interactions

$$= \frac{(Sx_1)^2 + (Sx_2)^2 + (Sx_3)^2 + (Sx_4)^2}{\text{No. of students per group}} = \text{c.f.}^* = \frac{293^2 + 183^2 + 322^2 + 196^2}{10} - \text{c.f.}$$

$$= 26143{\cdot}8 - 24700{\cdot}9 = 1442{\cdot}9.$$

Total s.s. of deviations within groups is the difference between the above two values, or 2602·2.

[1] A purely hypothetical example has been given, with the same number in each group, as in the actual material used the numbers in the groups were unequal, and this slightly complicated the calculations. An example of the calculation for groups of unequal numbers will be found in (2), section 69.

* c.f. (correction factor) is for sums of squares always the quantity $\frac{(Sx)^2}{N}$, and for sums of products the quantity $\frac{Sx.Sy}{\text{No. of pairs}}$.

Table 2

	Test I		Test II		
	x	x^2	y	y^2	xy
1938 class: Hindi speaking	40	1600	20	400	800
	30	900	22	484	660
	35	1225	18	324	630
	20	400	15	225	300
	38	1444	14	196	532
	42	1764	30	900	1260
	32	1024	32	1024	1024
	24	576	20	400	480
	20	400	18	324	360
	12	144	8	64	96
	293	9477	197	4341	6142
Non-Hindi speaking	25	625	30	900	750
	20	400	35	1225	700
	15	225	40	1600	600
	28	784	20	400	560
	30	900	22	484	660
	10	100	25	625	250
	15	225	15	225	225
	20	400	28	784	560
	12	144	24	576	288
	8	64	35	1225	280
	183	3867	274	8044	4873
1939 class: Hindi speaking	35	1225	20	400	700
	30	900	25	625	750
	40	1600	15	225	600
	42	1764	10	100	420
	45	2025	20	400	900
	40	1600	18	324	720
	25	625	25	625	625
	30	900	22	484	660
	20	400	24	576	480
	15	225	25	625	375
	322	11264	204	4384	6230
Non-Hindi speaking	12	144	20	400	240
	20	400	18	324	360
	18	324	18	324	324
	15	225	25	625	375
	25	625	20	400	500
	20	400	25	625	500
	24	576	18	324	432
	30	900	15	225	450
	20	400	12	144	240
	12	144	20	400	240
	196	4138	191	3791	3661
Grand total	994	28746	866	20560	20906

Partialling out sums of squares of deviations from the mean for test II

Using the same method as for test I, we get the following values:

Total s.s. of deviations from the mean

$$=20560-18748\cdot9=1811\cdot1.$$

Total s.s. of deviations attributable to group differences, etc.

$$=19198\cdot2-18748\cdot9=449\cdot3.$$

Total s.s. of deviations within groups $=1361\cdot8$.

Partialling out sums of products of deviations from the means for both tests

Total s.p. of deviations from the means

$$=S(x-\bar{x}).S(y-\bar{y})=S(xy)-\frac{Sx.Sy}{\text{No. of pairs}}=20906-\frac{994.866}{40}=-614\cdot1.$$

Total s.p. of deviations attributable to group differences and interactions

$$=\frac{(Sx_1.Sy_1)+(Sx_2.Sy_2)+(Sx_3.Sy_3)+(Sx_4.Sy_4)}{\text{No. of pairs of students per group}}-\text{c.f.}$$

$$=\frac{(293.197)+(183.274)+(322.204)+(196.191)}{10}-21520\cdot1=-421\cdot4.$$

Total s.p. of deviations within groups $=-192\cdot7$.

Calculating the Pearson product moment correlation for the two tests using total values before s.s. *and* s.p. *for group differences, etc. have been partialled out*

$$r_{12}=\frac{Sd_1d_2}{\sqrt{\{S(d_1^2).S(d_2^2)\}}}=\frac{-614\cdot1}{\sqrt{(4045\cdot1.1811\cdot1)}}=-0\cdot2269.$$

Calculating the Pearson product moment correlation for the two tests after the s.s. *and* s.p. *for group differences, etc. have been partialled out*

$$r_{12}=\frac{Sd_1d_2}{\sqrt{\{S(d_1^2).S(d_2^2)\}}}=\frac{-192\cdot7}{\sqrt{(2602\cdot2.1361\cdot8)}}=-0\cdot1024.$$

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NOTE BY A REFEREE

The device explained in Prof. Taylor's short paper, which has been in use in at any rate one educational psychology laboratory for some years, can be perhaps more concisely expressed thus. When the individuals tested fall into groups, and the correlation desired is that free from the influence of group differences, it is best to calculate the sums of squares and of products *separately for each group from its own means* (as would naturally be done as the experiment proceeded) and then simply to add these.

Thus in his own example of four groups of ten students each, the calculation on these lines would appear as follows, where 892·1 is $9477-293^2/10$, 369·9 is $6142-293\times 197/10$, and so on:

	Σx^2	Σxy	Σy^2
Hindi 1938	892·1	369·9	460·1
Non-Hindi 1938	518·1	−141·2	536·4
Hindi 1939	895·6	−338·8	222·4
Non-Hindi 1939	296·4	−82·6	142·9
Totals	2602·2	−192·7	1361·8

$$r = -192{\cdot}7/\sqrt{(2602{\cdot}2 \times 1361{\cdot}8)} = -0{\cdot}1024.$$

CRITICAL NOTICE

By ROBERT H. THOULESS

The Factors of the Mind: an Introduction to Factor-analysis in Psychology. By Cyril Burt. London: University of London Press. 1940. Pp. 509. 21*s.* net.

It is a fortunate circumstance for the development of psychology in this country that the outbreak of war led Prof. Burt to put into the form of a book the fruits of his unique knowledge of factor analysis which have been previously only fully available to the students of University College, London, and in a scattered and incomplete form for other psychologists in his articles on the subject in the technical journals. While there are now several books on the subject of factor analysis, it has been difficult for the student of psychology to gain a satisfactory understanding of the subject, since most of these have been written primarily in defence of the methods of their authors, and their tendency to deny any 'psychological meaning' to rival methods may well have led the not very sympathetic enquirer to doubt whether any of the methods had sufficient psychological meaning to be worth studying. Burt has his own contributions to make to method, but gives a sympathetic and understanding account of other methods which he regards as different but not necessarily erroneous ways of achieving what is virtually the same result. So his book succeeds in giving the student not merely an introduction to a particular method of factor analysis but an understanding of the whole subject.

The point of factor analysis may be expressed as an attempt to describe the abilities of any individual as so much intelligence, so much verbal ability, and so on. In other words, to specify an individual's abilities as $Ag+Bv+\ldots$, where g, v, etc. stand for abilities and A, B, etc. are numbers showing the amounts of each different ability possessed by the individual. Obviously, the same method may also be applied to other mental measurables than ability, and it may be equally possible to express his temperament as $Ma+Np+\ldots$. Also the analysis provides a method of specifying the abilities required in any particular test as $A'g+B'v+\ldots$, where A', B', etc. are test saturations with the abilities in question. One problem of factor analysis is, what are the factors, g, v, etc., that we ought to use for this purpose? Another is to devise methods by which amounts and test saturations of these factors can be estimated.

As a branch of psychological study, factor analysis has suffered from the opinion that it is an isolated branch of psychological research little related to the main current of psychological investigation and of such mathematical difficulty that it can only interest those with exceptional mathematical ability. It must be admitted that the quantitative determination of factors does require some knowledge of appropriate mathematical methods, although the difficulty of acquiring this knowledge is much less than is commonly supposed. The general principles of thought about factors, on the other hand, are, as Burt points out, neither new nor unfamiliar. It is a way of thinking found very generally in psychology and was known a good deal earlier than the special methods for quantitative estimation of factors. Burt gives two examples of earlier ways of thinking in terms of factors, although the term 'factor' was not used in them. The first is Wundt's tri-dimensional theory of feeling which was essentially an analysis of the complexity of feeling states into three orthogonal bi-polar factors. The second is the attempt of Helmholtz and other experimentalists to explain colour sensations as combinations of different amounts of a limited number of elementary or primary colours.

Perhaps the essential familiarity of the factor concept can be even better realized if we consider that it is found not only in psychology and related sciences but even in popular thinking. It is fundamental to popular political classification. We divide men into conservatives and radicals. This is a general bi-polar factor. We recognize that a man may be conservative or radical to any degree and also that we may say that a proposed measure is extremely conservative or radical, or somewhat so; in other words, we make the distinction between the amount of the factor possessed by an individual and the factor saturation of an Act of Parliament (which is here analogous to a mental test). We recognize, too, that the factorization into conservative and radical does not account for the whole of the political difference between men. We find some conservatives and some radicals who believe in establishing their ideals by violence (fascists and communists) and some of both parties opposed to violence. So more of men's political differences can be expressed by reference to this additional orthogonal factor, and if further exactness in the description of political differences were required, this could be attained by reference to other political factors. Furthermore, it would be generally recognized that the analysis of political differences might be made in other ways. The first fundamental distinction might be between individualists and socialists, a system of classification correlated with but not identical with the distinction between conservatives and radicals. From this starting-point, a different system of factors could account for the same political differences as those accounted for by the system of factors starting from the conservative-radical factor. This is exactly analogous to the different systems of factorization of abilities or other mental functions obtained by rotation of axes. There seems to be no sufficient reason for regarding the way of thinking in terms of factors as strange either to psychology or even to our more ordinary thinking. The novelty is rather in the application of precise quantitative methods to determine possible systems of factorization.

The early part of Burt's book is an admirable discussion of the purpose of factor analysis. It is possible that this would more appropriately have followed the description of methods, since it is only possible to understand the theoretical questions involved if the reader has already understood the practice and the results of factor analysis. The question of order is not, however, important, since it is to be hoped that the reader after finishing the book will return to the earlier chapters, which may then be read with increased insight.

The methods of factor analysis are a natural extension of Spearman's pioneer work on the general factor. Although modern workers in factor analysis have abandoned Spearman's original methods and at least some modern factorists reject his principal conclusion of the importance of the general factor, it remains true that his work is the foundation on which later work has been built, and the diversity of modern methods and even of modern conclusions shows how fruitful for psychology has been his original hypothesis.

Although Spearman was far from excluding the possibility of 'group factors' common to groups of abilities, his primary problem was to demonstrate that most of the correlation between abilities could be accounted for by one general factor common to all abilities and specific factors peculiar to particular abilities or at any rate common only to very closely related abilities. In other words, the equation with which he was primarily concerned was the two-factor equation $T=Ag+Bs_r$, where g is the general factor and s_r is the specific factor for the particular test in question. For this purpose, he devised the method of determining all possible tetrad equations between tests, and demonstrating that they did not differ significantly from zero.

Although laborious, this procedure involved no unfamiliar mathematical methods. When, however, correlations between larger numbers of subjects had shown that group factors could not be ignored and when large numbers of tests were used in a single research, it became necessary to use mathematical methods more economical in time than the calculation of innumerable tetrad differences. The required methods were found in matrix algebra.

These methods were not familiar to many psychologists, and they have not always been very lucidly explained by those psychologists who have used them. One of the demands, therefore, which may reasonably be made of Burt's book is that it should provide sufficient guidance as to the methods used for any student of psychology to be able to master them for himself. This requirement it fulfils very well. There are adequate examples fully worked out, and with so much knowledge of matrix algebra as can be obtained in a few hours' study of the more elementary parts of any text-book of the subject (such as A. C. Aitken's ***Determinants and Matrices***), and by working out for himself some factor analyses under the guidance of Burt's book, any psychologist without any particular mathematical gifts should be able to understand the essential methods of factor analysis.

There is more than one possible view of what aim factor analysis sets out to achieve. When Spearman showed that, within the limits of error of his results, a matrix of correlation coefficients of dissimilar tests could be analysed into the effect of one general factor common to all the tests and a number of specific factors peculiar to each, the conclusion he drew was that this general factor was a unitary power of the mind. If later research with larger numbers of cases (and therefore smaller errors) had confirmed the fact that only one factor common to a number of dissimilar tests could be found, the evidence for this conclusion would have been overwhelmingly strong.

It is now, however, generally agreed that a matrix of correlation coefficients between tests cannot be explained completely by the two-factor equation, and that other factors common to many or all of the tests are to be found in addition to Spearman's general factor. Moreover, any matrix of correlation coefficients can be analysed into factors in an indefinitely large number of ways which are mutually derivable from one another by a process which we can represent geometrically as a rotation of axes or algebraically as a post-multiplication by an orthogonal matrix. By this process, a system of factorization showing a large general factor may be converted into one with equal mathematical validity in which there is no factor common to all the tests. It may well be that Spearman was right in believing that his general factor does correspond to a unitary mental capacity, but it does not seem any longer possible to maintain that this can be proved by any statistical analysis. This is also true of the factors revealed by a process of multiple factor analysis. If a real meaning can be attached to the statement that intelligence, verbal capacity, etc. are unitary mental capacities, it remains true that this unitariness cannot be proved by factor analysis, and also that we cannot know that the proper figures for test saturations with these unitary capacities are those obtained by the use of one of the possible ways of analysing the original correlation matrix.

One way out of this difficulty is to find some ground for asserting that a particular method of analysis is the one right way which gives a real picture of the structure of mental abilities. Thus Thurstone believes that if, after a centroid analysis, the axes are rotated in such a way as to reduce to a minimum the number of negative saturations, and to increase as far as possible the number of zero saturations, factors will be obtained which correspond to real unitary mental capacities. This belief depends on the somewhat subjective criterion that the factors so obtained have more 'psychological meaning' than have other possible factor systems. Burt adopts the more radical alternative of abandoning the aim of distinguishing unitary mental capacities as a goal of factor analysis and regards his factors rather as analogous to the lines of latitude and longitude on a map. They "are to be thought of in the first instance as lines or terms of reference only, not as concrete psychological entities" (p. 18). This does not, however, mean that the factors are without scientific value or validity. "In order to give an adequate description of persons we must first discover in what *independent* directions a person may vary, and, at the same time, so far as possible, choose the direction so that each may carry with it a maximum amount of *dependent* variation" (p. 18). The last clause seems to be not quite exactly worded. The condition required for the greatest usefulness in description (and that actually secured by the method of weighted summation preferred by Burt) is not that each factor shall carry a maximum amount of dependent variation but that any small number of factors shall carry the maximum possible amount of dependent variation which can be carried without increasing the number of factors. This means that the maximum amount of variance is accounted for by the first factor, the maximum amount of the remaining variance by the second, and so on. This means that a general factor is retained in Burt's analysis and that it accounts for as much of the test results as can be explained by a general factor. This retention does not, however, necessarily imply that there is any psychological reality in the general factor, since it may be sufficiently justified as a measure of practical convenience.

The requirement that the greatest possible amount of the variance shall be accounted for by the smallest possible number of factors is obviously the most convenient condition to make for a purely descriptive analysis. It is, indeed, one that would commonly be recognized in everyday life. If we return to the consideration of the factor analysis of political differences, we see that two advantages may be claimed for making a first analysis into a liberal-conservative factor: (*a*) that it corresponds to a real social fact since there are actually liberal and conservative parties, and (*b*) that the differences between liberalism and conservatism may be considered to account for most of the political differences between men. A social theorist might write an article for a technical journal which began with the sentence: "There is a more fundamental difference between the political outlook of men than that expressed by the distinction between liberal and conservative." This new distinction might be (let us say) that between individualism and collectivism. In the language of factor analysis, he is saying that more of the variance in political beliefs between men is accounted for if we rotate the axis of our first factor from the liberal-conservative to the individualist-collectivist position. It would generally be agreed that if the new first factor really accounted for more of the political differences between men, it might be more convenient to adopt this as the first method of distinguishing between men's politics even though this meant giving up the advantage of making our principle of discrimination coincide with the social reality of the actual direction of party organization.

It may seem to some that Burt makes a disappointingly modest claim for a technique which is often regarded as the one scientific method of finding out what are the unitary constituents of the mind. To this objection, the reply may be made that the usefulness of lines of latitude and longitude is not dependent on any correspondence they may have with real characteristics of the globe. Moreover, the problem of what are unitary constituents of the mind (if it is agreed that this is a real problem) cannot be solved independently of the results of factor analysis. If factor analysis cannot, by itself, tell us what are the unitary capacities of the mind, it can tell us what are and what are not possible ways of making an analysis of mental capacities.

The problem of alternative factor analyses can perhaps best be made clear by taking an artificial numerical example. Let us suppose that four tests were used (a smaller number, of course, than would be used in practice for factor analysis). These may be supposed to be a test *PG* of plane geometry, *AR* of arithmetic, *AL* of algebra, and *SG* of solid geometry. Suppose that the correlation coefficients between these tests were those of the matrix given below:

	PG	*AR*	*AL*	*SG*
PG	—	0·685	0·42	0·51
AR	0·685	—	0·52	0·385
AL	0·42	0·52	—	0·21
SG	0·51	0·385	0·21	—

Analysis by Burt's method, in which as much of the difference between testees as possible is accounted for by the first factor, gives the following matrix of factor saturations:

	I	II
PG	0·87	0·21
AR	0·83	−0·19
AL	0·56	−0·29
SG	0·52	0·26

This seems to show a general factor (I) common to all four tests but most strongly operative in plane geometry, and a factor (II) which is bi-polar (showing both positive and negative test saturations) and which shows the greatest influence on solid geometry and the maximum negative influence on algebra. A natural interpretation of this would be that it was the contrast between test performances dependent on skill in spatial apprehension and those dependent on manipulation of symbols.

The objection has, however, been made by Thurstone and others that negative saturations imply a capacity which makes a performance worse if testees possess that capacity and that they can therefore have no reasonable psychological meaning. To this, Burt makes two replies. First, that the objection could only be valid if it were admitted that a factor must measure a real psychological entity, but if we regard a factor as a line of reference, there seems to be no reason why we should not choose a line of reference which marks a contrast between two abilities. Secondly, that apart from all quantitative factor analysis, we do use just such bi-polar terms in psychology. An obvious example is introversion-extraversion. Either can be treated as the positive pole, but any measurement positively saturated with one must be negatively saturated with the other.

If, however, we feel that a factor analysis can only have psychological meaning if there are no negative saturations, we can, in this particular case, very easily get rid of negative saturations by rotating the factor axes through 45°. This will then give a new set of factor saturations:

	I′	II′
PG	0·465	0·76
AR	0·72	0·46
AL	0·60	0·19
SG	0·18	0·555

Now we have two new factors both of which are, to some extent, common to all four tests but one of which (I′) is especially characteristic of arithmetic and to a somewhat less extent of algebra, while the other is found most strongly in plane geometry and not quite so much in solid geometry. If anyone chose to make his factor analysis in this way, he would probably call I′ a factor of numerical ability and II′ a factor of spatial apprehension, and suggest that both factors entered to some extent into all four tested operations.

A rotation through 45 % would, however, be somewhat arbitrary, since other rotations would equally well achieve the aim of getting rid of negative saturations. Let us suppose, therefore, that we wish to get the more rigidly defined solution of an analysis in which there are as many zero saturations as possible. In this particular case, we can only get zero saturations in two of the factors by increasing the number of factors above the minimum number of two and retaining also a general factor common to all tests. By appropriate rotations of the axes in three dimensions, we can get the analysis:

	I″	II″	III″
PG	0·76	0·44	0·17
AR	0·70	0·17	0·45
AL	0·46	0	0·43
SG	0·46	0·36	0

The psychological explanation is now likely to be that I″ is a factor of general ability, II″ a factor of ability to deal with spatial relations, and III″ a factor of ability to deal with symbols.

We now have three solutions of the original problem and many others might have been obtained. All are genuine solutions in the sense that if one had such factors in such saturations, the original matrix of correlations would have been obtained. This is shown by the fact that each matrix of saturation coefficients post-multiplied by its transpose gives (apart from its principal diagonal) the original matrix of correlation coefficients.

Close examination of these and other possible solutions shows, however, that they are less diverse than might appear at first sight. Their most essential features are common to all. In all, there is a tendency for arithmetic and algebra to resemble each other and for solid geometry and plane geometry to resemble each other. In the first analysis this tendency appears as the positive and negative saturations respectively of the two pairs of tests with a single bi-polar factor; in the second and third analyses it is shown as a high saturation of the two pairs of tests with two separate factors. In all analyses the possibility of a general factor is shown; in the first and third this appears as a separate general factor, in the second as a positive saturation of all tests with each of the two factors.

There are certain features of the matrix of factor saturations which remain invariant through all rotations of axes. These invariant features may be regarded as genuine characteristics of the relationship between the tests, revealed in different ways by all methods of analysis. If we regard factors merely as axes of reference for the quantitative description of tests, it is reasonable to regard all different analyses obtained by different rotations as equally valid for this purpose, although all may not be equally convenient in practice. There may still be argument as to the equal validity of analyses obtained by different methods of filling up the missing principal diagonal of the original correlation matrix—by inserting reliability coefficients, or unity (Hotelling), or by successive approximation to the sum of the squares of the corresponding saturation coefficients (Burt), etc.

We can perhaps best think of a factor as the system of measurements given by an imaginary test battery. The possibility of alternative factor analyses of the same matrix of correlation coefficients means simply that there is more than one system of imaginary tests which could give a complete picture of the results of the actual test measurements from which the correlation matrix was constructed.

The question of what are the right factors to use in factor analysis arises, however, if we believe that there are such things as unitary mental capacities and that it is desirable to use factors which correspond as closely as possible with these unitary capacities. In any case it arises as a question of practical convenience if we want to make any comparison between the results of factor analysis of different samples of tests by different investigators or to make any sort of practical use of the factors distinguished.

A possible use of the results of factor analysis in test construction is to make batteries of tests which are saturated as highly as possible with the factors obtained by any one method of factor analysis. In other words, test batteries are constructed which correspond as closely as possible to the imaginary test batteries which are the factors. This has, of course, been done in the construction of tests saturated as highly as possible with *g*, with *p*, with *v*, etc. Unless, however, we have reasons outside the results of factor analysis for deciding what really are the unitary abilities which ought to be measured by tests, there must be some element of arbitrariness in the selection of what system of factors is to be chosen as the basis of measurement. It is obviously not completely arbitrary since, although alternative directions of measurement are allowed by the possibilities of alternative methods of factor analysis, a much larger number of directions of measurement are excluded since they correspond to no possible system of factor analysis.

One way of dealing with this element of arbitrariness is to admit its presence and to define the factors to be used for measurement as the abilities measured by certain batteries of tests. Thus we can make a battery of tests to measure *g* and define *g* as the capacity measured by that battery of tests. Similarly, we can define *p*, *v*, etc., bearing in mind always the limitation that the system of capacities so defined must be consistent with some system of factor analysis. It is not necessary to make any assertion of the independent reality of the capacities so defined. This way of defining factors has been suggested recently. If adopted, it would be a reversion to the method adopted in the early days of factor analysis by W. P. Alexander in his monograph on *Intelligence, Concrete and Abstract*. Such a practice (which is equivalent to using a real test battery instead of an imaginary one to supply the lines of reference in a factor analysis) would be more economical of effort than the present method of every research worker making an analysis of the particular set of tests on which he is working into its own factors. The present method also has the disadvantage that the factors obtained depend on the particular sample of tests with which the work has been carried out; geography would be a very complicated subject if those who studied different parts of the earth's surface used different systems of lines of latitude and longitude.

An important feature of Burt's book is its adequate treatment of the problem of the statistical significance of factors. There is a considerable amount of published factor analysis in which this problem is disregarded or treated by quite inadequate methods, and factor saturations have been published when the numerical evidence for the existence of the factor in question has been very slender. It has probably often been the case that the later factors in published analyses have been characteristics only of residuals which are merely random collections of figures. Burt's interest is in scientific research and not the working out of mere arithmetical puzzles, and he provides adequate safeguards against this source of error. While, however, he employs means of testing whether there is statistically significant evidence for the existence of a further factor at any stage of the analysis, it is unfortunate that no means seems to be provided for estimating the limits of reliability of the saturation coefficients calculated for a factor which is proved to be really present. Thus when it is said that the saturation of a particular test with a particular factor is 0·372, one does not know whether this means that there is sufficient evidence that the value lies between 0·370 and 0·374 or whether it is only known to lie between 0·2 and 0·5. Perhaps factor analysts will supply this need in the near future.

The few defects which dim the lustre of this book are probably to be attributed to the haste in its preparation for the press which was necessitated by the outbreak of war. Some of the physical analogies in the early chapters seem to have been not very happily chosen. There are occasional patches of unnecessary obscurity, as, for example, the treatment of analysis of variance. Sometimes, there is a looseness of phrasing, as when, on p. 379 and elsewhere, the author speaks of the 'variance' of a trait. The mathematical term 'variance' can properly only be applied to a measurement, not to the thing measured, and it may be dangerously misleading to ignore this distinction in psychological measurements, since it leads to an overlooking of the arbitrary element in every psychological measurement.

There are many similar points with which a critic might quarrel, and perhaps one of the services of Burt's book when more normal times return will be to stimulate controversies which will elucidate many of the obscure points of factor theory. If there are defects, these are trivial; the merits of the book are great. It may safely be judged to be one of the most important contributions to psychology made during recent years.

PUBLICATIONS RECENTLY RECEIVED

The Second Yearbook of Research and Statistical Methodology. Edited by OSCAR KRISEN BUROS. Highland Park, New Jersey: The Gryphon Press. 1941. Pp. xx + 383. $5.

This book is a second and much larger volume on the same lines as *Research and Statistical Methodology Books and Reviews*, 1933–1938, under the same editorial. It consists of a list of some 359 books on research and statistical methods published in 1933 or after. For each book is given a selection of extracts of critical reviews which have been published in various scientific and academic journals. The books reviewed are listed alphabetically under authors, and include a great variety of technical literature, comprising (to take a random sample) such subjects as Agriculture, Education and Psychology, Marketing, Plant Diseases, Vital Statistics and Zoology. The volume should be of considerable use to students in many fields of study who wish to know what reviewers think of books which they contemplate reading. Extracts from reviews appear to have been carefully and impartially made, so that adverse criticism as well as praise is recorded. Wherever known the names of reviewers are also given, which is in some cases a guide to the respect one may show some particular review. Another useful point about the volume is that the student may learn what books have been published recently in the particular branch of learning which is engaging his interest.

Should the volume be sufficiently well received and adequate financial aid be forthcoming, the editor proposes to make three major improvements in the next edition. He suggests including books written in foreign languages, abstracts of articles appearing in scientific periodicals and critical reviews made by "able and frank-speaking specialists" of scientific articles and papers not appearing in book form. These improvements, especially the last two, should increase the value of the publication greatly. It is particularly the case in statistical literature that new techniques are often described in journals which are not always readily available to students and there is frequently a considerable time lag before such methods are introduced into text books and become widely known. The inclusion of the essentials of such articles in one volume would greatly expedite the learning of new methods by a larger population of students and research workers.

The present volume is well printed on good paper and, once one has mastered the indexing system, is easy to use.

The Psychology of Conversation. By T. H. PEAR. Discussion Books No. 51. London: Thomas Nelson and Sons, Ltd. 1939. Pp. 171. 2*s*. 6*d*. net.

In the introduction to this book Prof. Pear says that "psychologists ought not to avoid problems of human behaviour because they are popularly interesting". This theme he has developed at some length in a more recent paper in this *Journal*. That psychologists do to some extent avoid direct treatment of problems such as that of 'conversation' (except between children) is in a sense true. But I think this may be due to the fact that many psychologists doubt their ability to advance their study of the subject *beyond* the stage of popular interest, where it may be left to the novelist and the essayist. If the psychologist is to tackle it usefully he must be prepared, first, to analyse it according to its underlying causes, and secondly, to systematize these causes in accordance with some general scheme of human behaviour and personality. Now apart from conversations, the main object of which is the exchange of ideas and information—and these are probably in the minority—conversation appears to have two main functions: (1) social: the attempt to establish a friendly social relationship, in which the *manner* (tone of voice, phraseology, etc.) is everything, the *matter* comparatively irrelevant; (2) ego-centric: the expression of the speaker's personality ('letting off steam'), often combined with the attempt to impress it upon others to a greater degree than they can impress their personalities upon him (this often occurs at committee meetings and scientific discussions). Clearly these, and any other, functions of conversation demand a differential approach and treatment. How they have developed in civilized society, and their relationships to the personalities of the conversationalists, provide a fascinating study. But it is likely to be successful only if it is guided by some systematic theoretical approach; just as some order has been effected among the chaos of dream material and neurotic phantasy primarily by means of the Freudian theory, even if the latter is more of a symbolic representation than a scientific statement of truth.

Prof. Pear has in this book produced most valuable Prolegomena to such a study, but the latter still awaits completion.

Heredity in Mental Traits. By N. N. SEN GUPTA. London: Macmillan and Co., Ltd. 1941. Pp. xii+207. 7*s.* 6*d.* net.

As a general survey of what is known on the inheritance of mental traits, this book should prove useful to the elementary student of the subject. It includes also a discussion of theories of heredity, and of the work of McDougall and others on the Lamarckian hypothesis. As a detailed study of exact scientific investigation of inheritance, it is less satisfactory. It was perhaps unavoidable that much of the data described should be anecdotal rather than fully substantiated, and the conclusions drawn from them necessarily negative in the main. But the work on inheritance of intelligence might have been treated in more detail, and references should have been given in full. There is, for instance, only a brief résumé of the work of Newman, Freeman and Holzinger on twins. Neither is there any allusion to that of Blatz on the Dionne quintuplets.

However, these shortcomings are perhaps inherent in the nature of the treatment accorded to the subject of heredity—the battleground for extreme environmentalists on the one hand, and of typologists and of uncritical believers in theories of heredity on the other. As yet, there seems little hope of any integration between the two approaches to the subject.

Criminal Youth and the Borstal System. By WILLIAM HEALY and BENEDICT S. ALPER. New York: the Commonwealth Fund (London: Humphrey Milford, Oxford University Press). 1941. Pp. vii+251. 8*s.* 6*d.* net.

Convinced that the treatment of young criminals in the U.S.A. is of a thoroughly unsatisfactory nature, the authors of this book made an investigation of the English Borstal system to determine if it was likely to prove more successful. Their investigation was unfortunately cut short by the outbreak of war, and it is not quite clear how thorough an investigation they were able to make. But they became convinced of the value of the system in reclaiming young offenders aged 16–25. After a brief outline of the American reformatory system and its shortcomings, they give a detailed description of the Borstal system, its methods, institutions and the association which is responsible for the after-care of those released on licence. Its humane nature, its care for the individual and its lack of stereotyped routine, command their admiration. The only serious criticism is of the absence of psychiatric diagnosis and the lack of psychotherapeutic treatment for the appreciable proportion of emotionally unstable and psychoneurotic individuals among the youths committed to Borstal.

The book gives a good objective account of the Borstal system. It is, however, a pity that it was not possible to carry out a longer study containing a more thorough psychological analysis of the system and its effect on the inmates of the Borstal institutions.

An Investigation of the Technique of Psycho-Analysis. Edited by EDWARD GLOVER. London: Ballière, Tindall and Cox. 1940. Pp. x+188. 10*s*. 6*d*. net.

Dr Glover, Director of Research to the Institute of Psycho-Analysis, wishes to systematize and correlate work on the technique of psycho-analysis. As a preliminary it was necessary to learn what techniques are actually employed. The results of the enquiry are published in this book.

To obtain the information a questionnaire, dealing with typical problems—methods of dealing with anxiety, problems of interpretation, transference, appointments and time-tables, fees, sex of the analyst, the relation of theory to practice, etc.—was sent to twenty-nine analysts in 1938. Twenty-four sent in replies. The results were then submitted to the Society for discussion and a supplementary questionnaire was sent out to gather information on further points. And the information so gathered is very interesting and valuable, even if questionnaire methods are suspect and though the number of 'subjects' is small. Large differences in technique are found due not only to the fact that the training of the analysts has not been uniform, but perhaps more especially because of the individual differences between the analysts themselves.

It is not possible, in a brief review, to indicate the points on which there is general agreement and on which there are differences in technique. Those who are interested must refer to the text, to which there is a very full index.

The Nursing Couple. By MERRELL P. MIDDLEMORE. London: Hamish Hamilton Medical Books. 1941. Pp. xvii+195. 7*s*. 6*d*. net.

There is little doubt that much might be done to validate or disprove the Freudian theories as to the function in the child's psychic development of the satisfaction or frustration it experiences during suckling, by making observations of the actual behaviour of infants during suckling, and relating it to subsequent development and especially to fantasy life. Dr Glover, in an Introduction to this book, ably expresses this point of view. The book itself describes clearly and interestingly observations on the behaviour during suckling of forty-six infants in the lying-in ward of a hospital; their differing modes of satisfaction and frustration; and how these were related to the behaviour and attitude of the mother. The deleterious effect of anxiety and nervousness in the mother was particularly observed. But perhaps because the author did not live to continue the work, no data as to the child's subsequent development were obtained. There are a number of observations about the effects on fantasy life such as: "If the mother bothered the child too much during unsatisfactory feeds..., he remained passive and shrinking....I conclude that the feelings of the baby at these times would dispose him later to make fantasies of an attack during feeding...." In other words, pure speculation, even if inherently probable. The two methods of approach—observation of actual behaviour, and subsequent reconstruction through fantasy expression and free association—have still to be integrated together.

The book, however, contains a great deal of information on the treatment of babies during suckling which should be invaluable both to the mother, and also to the doctor and nurse.

Introduction to Educational Psychology. By WILLIAM CLARK TROW. London: George Allen and Unwin, Ltd. 1939. Pp. xiii+417. 8*s*. 6*d*. net.

This book begins quite interestingly with a brief but informative treatment of individual needs, emotions and personality adjustments. After that it follows the usual routine of elementary text-books on psychology, and is only very moderately interesting—although the summaries to each chapter are almost uniformly good. It should serve its purpose as well as most elementary psychological texts. But since it is designed entirely for the American student, there seems little purpose in publishing it in this country.

The Individual in the Group. By FLORENCE M. SURFLEET. London: Allenson and Co., Ltd. Pp. 190. 6*s*. net.

This book consists of hints on how to run group discussions, meetings, poetry readings, etc., and includes poetry to be read. It may be helpful to some who are responsible for organizing such group activities. But if it was intended to assist people in expressing their feelings and ideas in the presence of others, the suggestions offered should have been more severely practical and concrete, and much less 'highfalutin'.

The book is undated.

The Collected Papers of Wilfred Trotter, F.R.S. Oxford Medical Publications. London: Humphrey Milford (Oxford University Press). 1941. Pp. v+194. 10*s*. 6*d*. net.

This book consists of the reprints of ten lectures, together with one article and a letter to *The Times*, by the late Wilfred Trotter. They deal mainly with general problems of thought and conduct connected with the study and practice of medicine; and though written with wisdom and urbanity, present no points of striking interest to the psychologist.

PROCEEDINGS OF THE BRITISH PSYCHOLOGICAL SOCIETY, 1941

GENERAL MEETINGS

April 17–21, 1941. EXTENDED GENERAL MEETING AT NOTTINGHAM

April 17. 1st Session.

Business Session, followed by:

1. Paper: "National Character." By M. GINSBERG.
   (Being Inaugural Address as Chairman of the Social Psychology Section.)
2. Motion: "That an Extraordinary General Meeting of the Society be held at 55 Russell Square on Saturday, May 31 1941, to authorize and take such action as may be necessary to secure the Incorporation of the Society."
   (Proposed by the President; seconded by the General Secretary; and discussed. See Session 11.)

April 18. 2nd Session:

"The Contribution of Factor Analysis to the Study of Cognitive Ability." By J. W. JENKINS.

"A Method of Measuring After-Images." By LL. WYNN-JONES.

"The Problem of Dark Adaptation." By B. SEMEONOFF.

3rd Session (Being a meeting arranged by the Education Section).

Business of the Section, followed by papers:

"Differentiated Treatment of Reading Disabilities." By F. J. SCHONELL.

"Scholastic Ability at Different Social Levels." By C. M. FLEMING.

4th Session (Being a meeting arranged by the Social Psychology Section).

Business of the Section, followed by papers:

"International Community." By MAXWELL GARNETT.

"Field Theory in Social Psychology." By W. STEPHENSON.

April 19. 5th Session (Being a joint Meeting with the Institute for the Study of Animal Behaviour).

Annual General Meeting of the I.S.A.B.: Business of the Institute, followed by papers and films:

"Enquiry into the Place of the Study of Animal Behaviour in the Universities." Report read by F. B. KIRKMAN.

"Varied Effort in Instinctive Behaviour." By E. S. RUSSELL.

Ciné-Films: "Animal Behaviour."

April 19, 1941. 6th Session:

"The Shelter Situation as a Focus for Skilled Psychological Observation." By Margaret Lowenfeld.

"The Mental Defective in the Army." By F. J. S. Esher.

"Performance Tests with 'Military' Defectives." By E. J. G. Bradford.

7th Session:

"A Case of Hysteria—a Contrast in Theory and Practice in the Two Wars." By John Rickman.

"The Soldier's Defences and the Public Attitude." By W. C. M. Scott.

8th Session.

Business of the Medical Section, followed by a paper:

"Somatic Conditioning of Air-Raid Reactions." By H. Crichton-Miller.

April 20. 9th Session:

"Undeveloped Frontiers of Psycho-Analysis." By T. H. Pear.

10th Session:

"The Nature of Shyness." By Hilde Lewinsky.

11th Session (Being an open Session for discussion).

Among the business under discussion the Motion concerning action to secure the Society's Incorporation was put from the Chair and carried unanimously (see Session 1, above).

May 31, 1941. Extraordinary General Meeting in London.

The following resolution was passed:

"That the Council be and it is hereby authorized and directed upon a Society with the name of The British Psychological Society being incorporated under the Companies Act 1929 as a Company limited by guarantee and not having a share capital and with a Memorandum and Articles of Association and Bye-Laws in the terms of the print submitted to the Meeting and signed for purposes of identification by the Chairman thereof (with such modifications (if any) as the Council may think fit) to give or transfer to the said incorporated Society the whole of the real and personal property belonging to the present unincorporated Society subject to the then liabilities of the said present Society and so that upon such transfer taking effect the present unincorporated Society shall be dissolved."

July 26, 1941. General Meeting in London.

Discussion of Shelter and Evacuation Problems.

Short papers:

Group A. Shelter Problems.

"A Theoretical Basis for the Study of Shelter Problems." By P. Senft.

July 26, 1941. "First-Aid Lectures in London Shelters: Mental Attitudes." By Hermia Mills.

"Children in Air-Raids of the 1914–1918 War: Their Reflections in Later Life." By G. W. Goodall.

"Psychological Problems of Shelterers." By M. I. Dunsdon.

"Shelter Life in Birmingham." By C. Burns.

Group B. Evacuation Problems.

"War Strain in Evacuated Children." By A. T. Alcock.

"Children's Drawings and the War." By A. H. Bowley.

"The Welfare of Problem Evacuee Children." By R. H. Adams.

"Factors affecting Success in Billeting Evacuee Children." By R. H. Thouless.

"Evacuation Problems and Suggested Remedies." By Ethel Dukes.

Group C. Morale.

"Researches on Effects of Air-Raids." By P. E. Vernon.

"Post-Blitz Morale." By Tom Harrisson.

"Biological Background of Morale." By C. B. S. Hodson.

"Routine versus the Blitz." By Betty Addinsell.

December 20, 1941. General Meeting in London (Being the first Meeting of the incorporated Society).

Business, followed by:

Discussion of Problems affecting the Under-Fives in Total War. Opening paper by C. Burt.

Short papers:

"Evacuation with Mothers." By Enid John.

"Evacuation without Mothers." By Ruth Thomas.

"Under-Fives in Bombed Areas." By Gwen Chesters.

SECTIONAL MEETINGS

Medical Section

The Section has held no individual meetings during the year, but has co-operated in arranging papers given at General Meetings, notably at the 6th, 7th and 8th Sessions of the Extended General Meeting, above.

Education Section

April 18, 1941. 3rd Session, Extended General Meeting (see above).

A joint meeting with the Industrial Section arranged for February 6 was cancelled owing to the speaker's illness.

The Section has co-operated in arranging papers at General Meetings throughout the year.

Industrial Section

February 27, 1941.	"Some Characteristic Interests of Schoolboys between 11 and 14." By W. H. N. Hotoph.
March 27, 1941.	"Some Remarks on Incentives to Work." By Marie Jahoda.
May 5, 1941.	"War-Time Grouses in Industry." By Winifred Raphael.
June 5, 1941.	"The Importance of Vocational Guidance for Neurotics in War-Time." By E. L. Trist.

Social Psychology Section

April 17, 1941.	Inaugural Address of the Chairman of the Section, given at the Extended General Meeting (see above).
April 18, 1941.	4th Session, Extended General Meeting (see above).

A week-end Discussion Meeting of the Section, to be held in Scotland, was planned, but arrangements proved impracticable.

The Section has co-operated in arranging papers given at General Meetings, and has issued five bulletins during the year.

BRANCH MEETINGS

Scottish Branch

November 9, 1940.	(Edinburgh.) "A Study of Schizoid Processes." By W. R. D. Fairbairn. "The Personality of the Very Young Child: a Study of the Factors which Help or Hinder a Satisfactory Adjustment." By A. H. Bowlby.
December 7, 1940.	(Glasgow.) "The Relative Efficiency of Different Lengths of Practice Periods in School Learning." By L. B. Young. "An Objective Method of Marking English Compositions." By R. F. Morrison and P. E. Vernon. "Fantasy and Imagination in the Perception of Pictures." By R. W. Pickford.
February 1, 1941.	(Edinburgh.) "Musical Ability and Appreciation." By H. D. Wing. "Recognition and Déjà Vu: an Experimental Approach." By O. L. Zangwill.
March 1, 1941.	(Glasgow.) Symposium on Dark Adaptation and Night Blindness: "Observations on Dark Adaptation in Psychotics, using the Rowett adaptometer." By W. Mayer-Gross. "Night Blindness as a Neurosis." By E. Wittkower and T. F. Rodger. "The Influence on Dark Adaptation of Music and of Temperament." By P. Lafitte and P. E. Vernon. "Dark Adaptation during Stimulation with Coloured Lights." By B. Semeonoff.

May 24, 1941.	(Carstairs.) Symposium on War Neuroses: Introduction: "The General Treatment of Cases of War Neuroses." By C. D. Bruce. "A Survey of 100 cases." By J. D. Sutherland. "Illustrative Case Material." By H. Gaussen.
November 1, 1941.	(Edinburgh.) "Compulsive Offences in Juveniles." By Denis Carroll. "A Study of War Attitudes." By P. E. Vernon. "Psychological Circumstances of Wars that Ended between 1820 and 1929 A.D." By L. F. Richardson.
December 6, 1941.	(Glasgow.) "Aspects of Musical Inspiration and Composition." By W. Gillies Whittaker. "Aspects of Pictorial Composition and Inspiration." By W. O. Hutchison.

Midland and Northern Branches

Neither of these Branches has found it possible to arrange individual Branch Meetings during the year, but a large number of their Members attended the Extended General Meeting at Nottingham, and several have contributed papers at this and other General Meetings of the Society. Both Branches suffer in war-time from the difficulty of arranging Meetings which can be attended by Members scattered over a wide area, but both are still in contact with local Members.

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BRITISH PSYCHOLOGICAL SOCIETY

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[1] Note. Subject to war conditions, the office of the Society, as from 1 July 1941, will be at the following address: Tavistock House South, Tavistock Square, London, W.C. 1. It is requested that notification of any errors in the List of Members be sent to Miss Addinsell at that address. All alterations for the next issue should reach the office before 1 April 1942.

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I. Straker, Dermot, B.A., Pucksridge, Crooksbury, Farnham, Surrey.
I. Turner, Miss E. M., Ravensworth, Mortimer, Berks.
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BRITISH PSYCHOLOGICAL SOCIETY

RULES

NAME AND OBJECTS

1. This Society shall be called the British Psychological Society.

2. The Society is instituted for promoting the co-operation of those interested in the various branches of Psychology.

3. The Society shall be responsible for the publication of *The British Journal of Psychology.*

MEMBERS AND SECTIONS

4. The Society shall consist of Honorary Members, Ordinary Members, Branch Associates and Sectional Associates.

5. Persons of scientific distinction who have contributed to the advancement of Psychology shall be eligible for election as Honorary Members.

6. Honorary Members shall be entitled to all rights and privileges of membership.

7. Honorary Members may be elected from time to time on the nomination of the Council at any General Meeting of the Society.

8. The Council shall be empowered to institute Sections of the Society, each Section being concerned with a special branch or aspect of Psychology.

9. Election to a Section or Sections shall *ipso facto* constitute election to the Society as a whole and entitle those so elected to receive notice of and to take part in all General Meetings of the Society in addition to the meetings of the Section or Sections to which they belong.

10. Sections shall have the right of electing Sectional Associates who shall receive notice of and shall be permitted to attend all Meetings of the Section or Sections to which they belong. Such Sectional Associates shall however take no part in the Government of the Society or the Sections, and they shall not receive *The British Journal of Psychology.*

10 *a*. Transfer from Sectional Membership to Sectional Associateship may take place on January 1st only, and after not less than six months' notice from the Member of his wish to transfer.

11. The procedure as regards the election of Sectional Associates shall be the same as that in the case of Members.

12. Every Candidate for election to the Society shall be recommended by at least two Members who shall be prepared to furnish

information as to the Candidate's qualifications for membership. No Candidate shall be submitted for election to the Society who has not first been approved by the Council or by a Sectional Committee.

13. A Candidate who has been approved by the Council or by a Sectional Committee shall be nominated for election at the next General Meeting of the Society, or at the next Meeting of the Section respectively.

14. One full week before the Meeting at which the ballot is to take place, the Secretary shall send to each Member a balloting paper containing the names and addresses of the Candidates and the names of their proposers.

15. Any Member unable to attend a Meeting at which a ballot is to take place can vote by sending his balloting paper to the Secretary.

16. One adverse vote in five shall exclude.

17. To vote *for* a Candidate, a Member must prefix a cross to the Candidate's name; to vote *against* a Candidate, he must erase the Candidate's name. Members who wish to record a vote neither for nor against a Candidate must leave the name untouched.

18. Any Member whose subscription remains unpaid for two years may, after receiving due notice, be removed by a resolution of the Council from membership of the Society.

19. Whenever it shall be proposed to remove a Member from the Society for any reason other than failure in payment of subscription, the matter shall be brought as a formal resolution before the Council or one of the Sectional Committees (whichever it shall most concern). In the event of the resolution being passed, a ballot shall be held for the removal of the said Member at the next General Meeting of the Society, or at the next Meeting of the Section respectively. Should two-thirds of the Members voting vote in favour of the removal of the Member, that Member shall be removed from the Society.

20. If at any time it is desired to abolish any Section or Sections, to subdivide any Section or Sections, or to amalgamate two or more Sections, a Special Meeting of the Section or Sections concerned may be called for this purpose according to the conditions laid down in Rule 44. Resolutions passed at any such Meeting shall be reported by the Sectional Secretary or Secretaries concerned to the next Meeting of the Council. Should the Council approve of such resolutions, such resolutions shall immediately take effect. But should the Council formally express disapproval of any or all of the said resolutions, these resolutions shall be considered by an Extraordinary General Meeting of the Society sum-

moned in accordance with the conditions laid down in Rule 44 and the decision of this Meeting shall be final.

In the case of the desire to abolish any Section or Sections, the following alternative procedure may be followed. An Extraordinary General Meeting of the Society shall be summoned in accordance with the conditions laid down in Rule 44, and any resolution passed at this Meeting regarding the abolition of the said Section or Sections shall be final.

OFFICERS, COUNCIL AND SECTIONAL COMMITTEES

21. The business of the Society as a whole shall be conducted by a Council consisting of the President, Deputy President, Vice-Presidents, Treasurer, General Secretary, Librarian, Editors of *The British Journal of Psychology*, one representative each of every Section and Branch to be nominated by the Sectional and Branch Committees, and of six Ordinary Members, and not more than two members co-opted by the Council.

22. The President and Deputy President of the Society shall hold office for three years. The other Officers of the Society shall retire annually but shall be eligible for re-election, the Deputy President shall be eligible only once for re-election, and the President shall not be eligible for re-election until after the lapse of a further period of six years. Every President shall at the termination of his period of office become a Vice-President for a period of six years. Co-opted members of the Council shall cease to hold office at the Annual General Meeting of the year of their election.

23. Every year two Ordinary Members of the Council, having served for three years, shall retire and shall be ineligible for re-election until after the lapse of one year.

24. Names of Members to serve on the Council may be suggested by the Council or by any Member by sending the proposed names to the General Secretary at least two weeks before the Annual General Meeting.

24 *a*. Nominations of representatives of the Sections and the Branches shall be sent to the General Secretary at least two weeks before the Annual General Meeting.

25. Nominations for the election of Officers shall be handed in to the General Secretary before the Meeting preceding the Annual General Meeting and be announced at the Meeting preceding the Annual General Meeting.

26. One full week before the Annual General Meeting, the General Secretary shall send to each Member a balloting list containing: (1) The names of the Council; (2) The number of the attendance of members of the Council at the Council Meetings held during the year; (3) The names of those proposed by the Council and Committees or by Ordinary Members to fill the vacancies.

27. The Society may fill all the vacancies among the officers that may occur between two Annual General Meetings by ballot, in accordance with Rule 15, at the next General Meeting, due notice having been given. Similar vacancies among the Ordinary Members of the Council shall be filled by co-option by the Council.

28. In the absence of an ex-officio Chairman (in accordance with the conditions laid down in Rule 42) the Council shall elect one of its Members to act as Chairman of Council. A quorum at a Council Meeting shall consist of five. If there be no quorum, the Members attending shall have power to transact business, subject to the approval of absent Members, to whom the resolutions adopted shall be forwarded.

29. The first Meeting of each Special Section shall be convened by the General Secretary of the Society. At this Meeting the Section shall appoint its Chairman, Secretary and Sectional Committee.

30. The Chairman and Secretary of a Section shall retire annually but shall be eligible for re-election.

31. The size of a Sectional Committee shall be determined by the Section. A quorum at a Meeting of a Sectional Committee shall be as that Committee shall decide.

32. When Sections have been formed, Rules 12, 13, 14, 15, 21, 24, 25, 26, 27, 28, 40, 41, 42, 43, 44 shall (*mutatis mutandis* and unless expressly stated to the contrary) apply respectively to the Committees, Chairmen and Secretaries appointed by these Sections.

33. The Society shall not make any dividend, gift, division or bonus in money unto or between any of its Members.

SUBSCRIPTIONS AND FINANCE

34. Each Ordinary Member shall pay an annual subscription of one guinea, which shall entitle him to receive a copy of *The British Journal of Psychology*, with the exception of such additional portions of the *Journal* as are devoted to special branches or aspects of Psychology and are published separately.

35. Ordinary Members who are members of all Sections shall pay an inclusive annual subscription of £3. 3*s*.

36. In addition to the Ordinary Membership Subscription Members shall pay an additional fee for every Section which they join. This additional fee shall entitle Members of any Section to receive such additional and separately published portions of *The British Journal of Psychology* as may be devoted to the special branch or aspect of Psychology studied by the Section. The amount of such additional fee shall be fixed by the Committee of the Section concerned in consultation with the Treasurer, and any resolution passed by the Sectional Committee with reference to such fee shall, before coming into force, receive the approval of the Council.

37. Sectional Associates shall pay a subscription the amount of which shall be determined by the Sectional Committee in consultation with the Treasurer of the Society.

38. The subscription is payable in advance and becomes due on January 1st. *The British Journal of Psychology* shall be sent only to Members whose subscriptions have been duly paid.

39. The Council shall grant to each Section permission to incur such expenses as may be necessary for the work of the Section; the approximate amount of such expenses to be determined by the Council.

MEETINGS

40. The Annual General Meeting shall be held in December, when the General Secretary's Report and the Treasurer's Statement of Accounts shall be laid before the Society.

41. The General Meetings of the Society shall be at such times and places as the Council shall decide, but in each year there shall be at least four meetings.

42. The President, Deputy President, or failing them a Vice-President, shall, whenever possible, act as Chairman at the Meetings of the Council and of the Society. In the absence of the President, Deputy President, or a Vice-President, some other Member shall, on the motion of the General Secretary or his substitute, be elected to act as Chairman.

43. The Secretary shall send to all Members of the Society notices of each Meeting at least seven days beforehand, and of the business to be transacted at the Meeting.

44. An Extraordinary General Meeting may be summoned at any time by the Council, or by the General Secretary on the written request of twenty Members; at least fourteen days' notice of such Meeting shall be sent to each Member of the Society.

VISITORS

45. Each Member shall have the privilege of introducing two visitors at any Meeting. The Council or a Sectional Committee shall however have the power of closing any Meeting or Meetings to visitors, provided notice to this effect be given on the notice convening such Meeting or Meetings. The names of visitors together with the names of Members introducing them shall be entered in a book provided for that purpose. Any Member of the Society attending a Meeting of a Section to which he does not belong must be introduced by a Member of that Section.

BRANCHES

46. The Council shall be empowered to sanction and dissolve the establishment of Branches of the Society throughout the British Empire: Members of such Branches shall pay the same subscription as that of Ordinary Members direct to the Treasurer of the Society and be entitled to the same rights and subject to the same rules as Ordinary Members. The Council shall be empowered to refund to the Treasurer of any Branch such proportion of Branch Members' subscriptions as the Council may determine to meet local expenses.

46 *a*. Any Rules made by Branches of the Society shall be first submitted to the Council for their approval.

46 *b*. The minimum number of Members necessary to form a Branch shall be 10.

46 *c*. Branches shall be required to present a report to each Annual General Meeting.

46 *d*. Branches shall have the right of electing Branch Associates under conditions similar to those governing the election of Sectional Associates.

RULES

47. The Rules of the Society may be altered at the Annual General Meeting, after notice of alteration has been given at a previous Ordinary General Meeting, or they may be altered at an Extraordinary General Meeting summoned for that purpose. Such Extraordinary General Meeting shall be held in London, and the purpose for which it is called shall be expressly stated in the notice convening the Meeting. No other business shall be transacted thereat.

BRITISH PSYCHOLOGICAL SOCIETY

NOTE. The Society's Library has been removed from London and is now housed for the duration of the war at University College, Nottingham, by kind permission of the Council and Senate of that College. The usual facilities for reference are available for those members able to use the Library at Nottingham. It is not possible to lend volumes.

Members who wish to confirm references or to obtain other information by post are asked to write to Mr G. Ellis Flack, M.A. (Librarian) at University College, University Park, Nottingham, and not to members of the University Staff.

LIBRARY RULES

1. The Library of the Society shall ordinarily be open for the use of members from 10 a.m. to 5 p.m. (Saturdays, 10 a.m. to 1 p.m.).

2. Persons other than members of the Society may use the Library only on obtaining permission from the Hon. Librarian. Such permission shall as a rule be granted only to scientists or research students who are known personally or by reputation to the Hon. Librarian, or who are introduced to him by a member of the Society. Such persons must in all cases have declared their willingness to conform to the rules regulating the use of the library by members.

3. Members must on each visit to the library sign the book provided for that purpose.

4. No book may be marked, annotated, mutilated, or defaced in any way.

5. A member shall be responsible to the Society for any loss or damage occurring to a book while in his use or possession.

6. No books may be removed or borrowed from the library.

LIST OF PERIODICALS AND SERIAL PUBLICATIONS CONTAINED IN THE LIBRARY (1941)

* Denotes publications from year given up to date.

Italics denote publications added to the Library since last publication of list.

*Acta Psychiatrica et Neurologica (with Supplements). 1938–
*Acta Psychologica. 1935–
*Action et Pensée (Bulletin de la Société Internationale de Psychagogie et de Psychothérapie). 1925–
*American Association on Mental Deficiency: Proceedings. 1936–
American Journal of Insanity. 1903–1915.
*American Journal of Orthopsychiatry. 1930–
*American Journal of Psychiatry. 1921–
*American Journal of Psychology. 1888–
*Annales médico-psychologiques. 1921–
*l'Année psychologique. 1909–
*Archiv fur die gesamte Psychologie. 1903–

Archiv für Gewerbepathologie und Gewerbehygiene. 1930–1932.
Archiv für Kriminologie. 1922–1930.
*Archives de psychologie. 1902–
*Archives of Neurology and Psychiatry. 1919–
*Archives of Psychology. 1907–
*Archivio generale di neurologia e Psychiatria. 1920–. (Now *Archivio de Psicologia, Neurologia, Psichiatria e Psicoterapia.* 1939–.)
*Australasian Journal of Psychology and Philosophy. 1923–
Berichte uber den 1–9 Kongress für experimentelle Psychologie. 1904–1924.
Brain. 1914–1921.
*British Journal of Educational Psychology. 1931–
*British Journal of Medical Psychology. 1920–
*British Journal of Psychology (General Section). 1904–
*British Journal of Psychology Monograph Supplements. No. 1–
*Buletin Eugenic si Biopolitic. 1936–
Canadian Journal of Mental Hygiene. 1919–1922.
*Catholic University of America Studies in Psychology and Psychiatry. 1926–
*Character and Personality. 1932–
Columbia University Contributions to Education. (Selected numbers.)
*Comparative Psychology Monographs. 1922–
Dictionary of Psychology (Warren).
Die Experimentelle Pädagogik. 1905–1910.
*Educational Research Bulletin. 1928–
*l'Encéphale. 1927–
*Eugenics Review. 1922–
Fortschritte der Psychologie. 1913–1922.
Forum of Education (*now incorporated with the* British Journal of Educational Psychology). 1923–1930.
*Genetic Psychology Monographs. 1926–
Grenzfragen des Nerven- und Seelenlebens. 1900–1926.
(The) Human Factor (Journal of the N.I.I.P.) (*now* Occupational Psychology). 1932–1937.
*l'Hygiène Mentale. 1927–
*Imago. 1913–
*Indian Journal of Psychology. 1926– (*set incomplete*).
*Industrial Health Research Board Reports. No. 1–
Industrial Psychology. 1926–1928.
*Industrielle Psychotechnik. 1924–
*International Journal of Psycho-Analysis. 1920–
International Zeitschrift fur Individualpsychologie. 1924–1937.
*Internationale Zeitschrift für Psychoanalyse. 1913–
Journal de Neurologie et Psychiatrie. 1927–1929.
*Journal de Psychologie. 1904–
*Journal für Psychologie und Neurologie. 1902–
*Journal of Abnormal and Social Psychology. 1906–
Journal of Animal Behavior. 1911–1917.
*Journal of Applied Psychology. 1917–

Journal of Comparative Neurology and Psychology. 1907–1910.
*Journal of Comparative Psychology. 1921–
*Journal of Educational Psychology. 1910–
*Journal of Educational Research. 1920–
Journal of Experimental Pedagogy. 1911–1921.
*Journal of Experimental Psychology. 1916–
*Journal of General Psychology. 1928–
*Journal of Genetic Psychology (*formerly* Pedagogical Seminary). 1891–
Journal of Juvenile Research. 1928–1938.
*Journal of Mental Science. 1921–
Journal of the National Institute of Industrial Psychology (*see* The Human Factor). 1924–1932.
*Journal of Nervous and Mental Disease. 1926–
*Journal of Neurology and Psychiatry (*formerly* Journal of Neurology and Psychopathology). 1938–
Journal of Neurology and Psychopathology (*see* Journal of Neurology and Psychiatry). 1920–1937.
*Journal of Parapsychology. 1937–
Journal of Philosophical Studies (*now* Philosophy). 1926–1930.
*Journal of Philosophy. 1911–
Journal of Philosophy, Psychology and Scientific Methods. 1908–1914.
*Journal of Social Psychology. 1930–
*Journal of the Royal Anthropological Institute. 1909–
*Kwartalnik Psychologiczny. 1930–
Man. 1906–1928.
*Mind. 1915–
*München: Arbeiten aus dem Psychologischen Institut der Universität. 1933–
Nervous and Mental Disease Monograph Series. (Selected numbers.)
*Neue Psychologische Studien. (München.) 1923–
*Occupational Psychology (*formerly* The Human Factor). 1937–
*Organon. 1936–
Pädagogisch-psychologische Arbeiten aus dem Institut des Leipziger Lehrervereins. 1911–1931.
*Pedagogical Seminary and Journal of Genetic Psychology (*now known as* Journal of Genetic Psychology). 1891–
*Personnel Journal. 1927–
*Philosophical Review. 1915–
Philosophische Studien (Wundt). 1883–1902.
*Philosophy (*formerly* Journal of Philosophical Studies). 1931–
Praktische Psychologie. 1919–1922.
Proceedings of the Aristotelian Society. 1916–1923.
*Proceedings of the National Academy of Sciences. 1929–
*Proceedings of the Society for Psychical Research. 1910–
**Psicotecnia*. 1939–
*Psyche. 1921–
*Psychoanalytic Quarterly. 1932–
*Psychoanalytic Review. 1913–

die Psychoanalytische Bewegung. 1929–1933.
*Psychological Abstracts. 1927–
*Psychological Bulletin. 1904–
Psychological Clinic. 1908–1935.
Psychological Register. 1929, 1932.
*Psychological Review. 1894–
Psychological Index (*now incorporated in* Psychological Abstracts). 1894–1935.
Psychological Index Abstract References: Vol. 1919–1928. (Presented by the Editor.)
*Psychological Review Monographs. No. 1–
Psychologische Arbeiten (ed. Kräpelin). 1895–1928.
*Psychologische Forschung. 1922–
Psychologische Studien (Wundt) (*see* Neue Psychologische Studien). 1906–1917.
*Psychometrika. 1936–
*Revista de Psihologie. 1938–
*Revue Française de Psychanalyse. 1927–
*Rivista di Neurologia. 1928–
*Rivista di Psicologia. 1905–
Science Progress. 1917–1933.
*Scientia. 1926–
*Smithsonian Institution Publications. 1928–
Training School Bulletin. 1929–1931.
*le Travail Humain. 1933–
University of California Publications. 1916–1932.
Yale Psychological Laboratory Studies. 1892–1902.
*Zeitschrift für angewandte Psychologie. 1908–
*Zeitschrift fur angewandte Psychologie. Beihefte.
Zeitschrift fur Neurologie und Psychiatrie. 1910–1914.
*Zeitschrift für padagogische Psychologie. 1899–
Zeitschrift für psychoanalytische Pädagogik. 1926–1937.
*Zeitschrift für Psychologie. 1890–
Zeitschrift fur Psychotherapie und medizinische Psychologie. 1909–1915.
Zeitschrift für Sinnesphysiologie. 1908–1938.

In addition to the above, the Library contains a few numbers only of the following publications:

Archives of Philosophy, Psychology and Scientific Methods. 1905–1906.
Archivos Brasileiros de Hygiene Mental.
Bulletin de l'Institut Général Psychologique. 1915–1933.
Journal of the Department of Letters, University of Calcutta.
Mental Measurement Monographs. 1927–1936.
Polske Archiwum Psychologji. 1926–1935.
Scandinavian Scientific Review. 1922–1924.
Tohoku Psychologica Folia.
Travaux Psychologiques de l'Université Jean Casimir de Lwów, Poland.

Human Nature and the Social Order (E. L. Thorndike). 1940. (Presented to the Society by the author and at present on loan for reference in the Library of the London School of Hygiene and Tropical Medicine.)